Volume XV. *December, 1902.* *Number 6.*

THE

PHYSICAL REVIEW.

A RELATION BETWEEN THE CHANGES OF LENGTH AND THE CHANGES IN THERMO-ELECTRIC POWER CAUSED BY MAGNETIZATION.

By Edward Rhoads.

I HAVE taken up the subject of the thermo-electric power of magnetized against unmagnetized iron in looking for some property of iron that would vary with magnetization in the same way as the length. The large amount of work[1] which has been done on changes of length with magnetization has shown that those changes which take place are not the strains which would be caused by the stresses of Maxwell's theory or any other. It seems, therefore, that they must be at least in part due directly to the orientation of the magnetic molecules. What then is more likely than that other changes due to the same cause should show a relation to them?

Several experimenters have placed thermal junctions in magnetic fields, and some have made the necessary provision that the magnetization of the iron member should be uniform and the "demagnetizing force" either negligible or calculable. I wish to refer only to Chassagny,[2] who discovered that the thermo-electromotive force of magnetized iron increases to a maximum and then begins to

[1] For full reference on this subject see Nagaoka's "Rapport presentée au Congrès International de Physique," Paris, 1900.

[2] Chassagny, Compt. Rend., May 1, 1893.

diminish as the magnetizing field is increased, and to Houllevigue[1] who found that the diminution continues in higher fields and that a reversal actually takes place. This suggests change of length and made me wish to work out the cyclic curve for comparison with it.

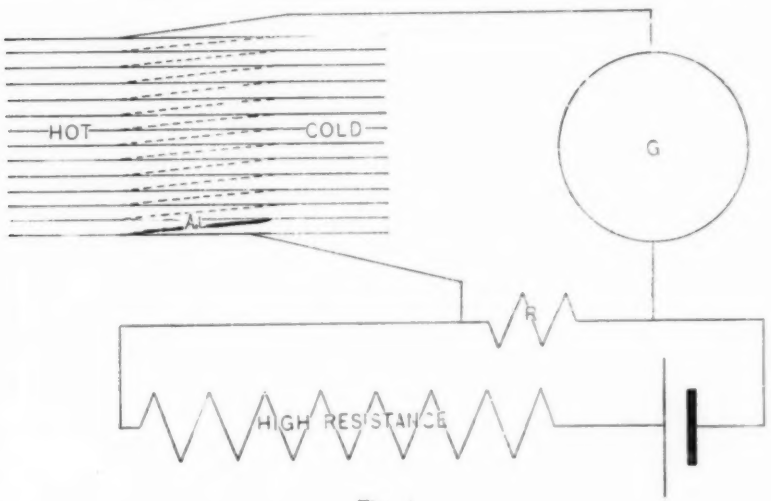

Fig. 1.

A thermopile was constructed as shown in the diagram, Fig. 1. The heavy lines represent the iron members, the light lines between represent the copper members, and the heavy dotted line represents a bar formed of an alloy of zinc and antimony in the proportion of their combining weights, discovered by Becquerel to be far beyond antimony at its own end of the thermo-electric series. Its function is to oppose the thermo-electromotive force of all the copper-iron junctions giving us a pile having no thermo-electromotive force on the whole, yet containing fourteen couples of iron against magnetically neutral substance. Of course there will be a fraction of one copper-iron couple left unopposed and this is neutralized by a small amount of potential in a battery circuit as shown in the diagram. The object of introducing the alloy instead of using the battery to oppose all the copper-iron couples is to obtain constancy of the zero.

The hot and cold chambers were each formed by two thin jackets between which the row of junctions were laid, and through the pair

[1] Houllevigue, Jour. de Phys., 3d series, Vol. 5, p. 53, 1896.

at one end steam passed, and through the pair at the other a stream of tap water. A standardized thermo-couple, to act as a thermometer, was also inserted with the thermopile. The whole arrangement was slipped into a solenoid wound on a flat tube of cast sulphur about 60 cm. long by 2 cm. by 6 cm. G is a low resistance Rowland-D'Arsonal galvanometer. The sensitiveness given by the arrangement is indicated by the fact that 10^{-9} volt per junction per degree centigrade difference in temperature gave above nine scale divisions deflection on galvanometer.

The solenoid had seven layers No. 18 copper wire, 66 C.G.S. per ampère, and was connected up with a suitable shunting device making it possible to vary the field continuously, — used on 110-volt circuit.

With this apparatus a cyclic curve was obtained which is shown in Fig. 2. As the field was increased, toward the left in the figure, the current flowed from unmagnetized member to magnetized through the hot junction; stronger and stronger at first, then reached a maximum and began to diminish. If now the field was gradually withdrawn the thermal current reached a maximum greater than the previous one, then fell a little, leaving a considerable "residual current" flowing when the magnetizing field was reduced to zero. This was only done away with when the field was reversed. When 200 was reached the field was again withdrawn, increased to 200 again toward the left and then to 500 toward the right giving the curve shown. Its interest lies in its similarity to and also its difference from change of length cycles for the same fields.

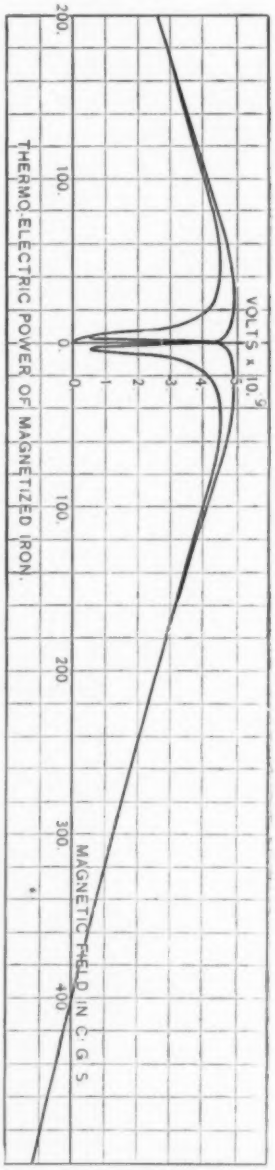

Fig. 2.

For purposes of comparison, therefore, I have plotted in Fig. 3 three changes of length curves. The first, marked "obs.," is a cyclic one taken from a previous paper in which the specimen was

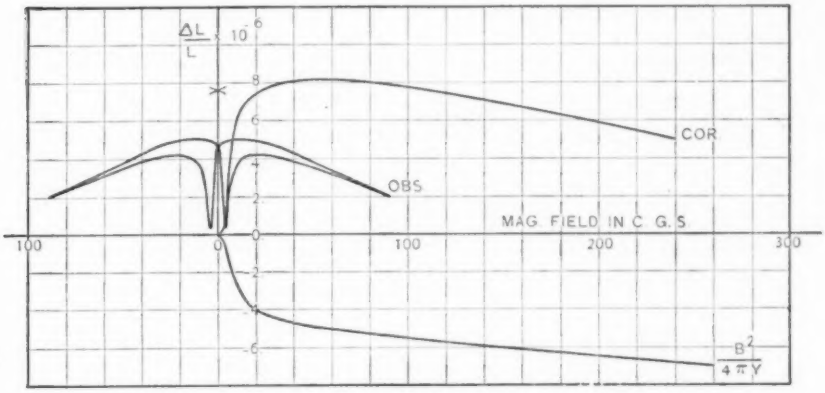

Fig. 3.

a different one of the same kind of iron. The curve would cross the line, *i. e.*, the specimen regain its original length, in a field of 150 or 160. For different specimens the curves differ, but this point always lies between 100 and 300 for soft and thoroughly annealed iron, such as we are dealing with.

The other two curves are taken from a paper by Dr. Brackett. The one marked $B^2/4\pi Y$ is a curve calculated by applying Maxwell's stress, $B^2/4\pi$ to the material. Y is Young's Modulus. Much has been said about the propriety of considering Maxwell's stresses as being actual mechanical forces tending to compress the iron. Reference may be found in the place cited above. One way of looking at the question is this: Suppose the iron wire is made up of thin discs alternately of north and south polarity separated by layers of non-magnetic matter. In this case there can be no doubt I think that the contraction takes place. If we pass on now to the case where molecular magnets are distributed in non-magnetic matter or held by any non-magnetic mechanism the same holds true. On the molecular theory then the contraction is definitely predicted. Professor Rowland was so sure of it that he believed in correcting the observed change of length for this correction. Such a corrected curve is shown marked "Cor." It may be considered as repre-

senting the unexplained change of length and must be ascribed to the orientation of the magnetic molecules.

Now it seems to be this curve that agrees with the thermo-electric power curve.

It is to be remembered that, as different specimens give varying change of length curves and also probably varying thermo-electric power curves, a comparison of this kind is not entirely satisfactory, and even if we were dealing with the same specimen in both curves the effect of non-isotropy or "grain" would still be likely to interfere with the comparison.

It is also to be noticed that the electromotive force of any thermo-couple is made up of two parts. That due directly to the difference in temperature of the junctions, and that due to the temperature fall along both of the members of the couple. In a more exact comparison it may be found valuable to separate these two parts. A slight correction also might be made for the effect of the temperature of the hot junction on its magnetization.

In nickel the change of length curve is of a different type from that of iron and the changes are about ten times greater. I have

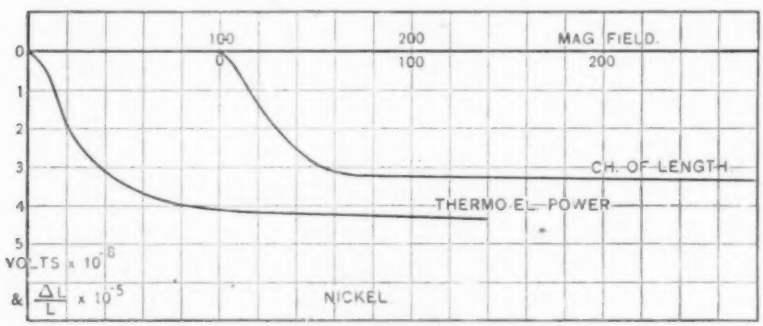

Fig. 4.

plotted such a curve in Fig. 4, the one to the right hand. It is taken from a paper by Nagaoka and unfortunately deals with an ovoid so that the demagnetizing force distorts it somewhat. To the left in the same figure I have plotted my thermo-electric curve for nickel obtained from a similar arrangement to that used for iron. The vertical scales of both these curves will be seen to be smaller

than the iron curves. The two properties turn out to be related in the opposite sense in the two metals. Whether the factor of proportionality is the same for both cannot be determined from curves which are not strictly comparable, as in this case.

A possible explanation of the close relation between the two properties suggested itself at once. Since we know that stretching or compression by ordinary mechanical means alters the thermo-electric power of a metal, it is possible that stretching or compression due to magnetization would do so in the same proportion. If the thermo-electric power turned out to be that called for on this hypothesis it would be necessary to consider that changes of length due to magnetism are ordinary mechanical strains due to magnetic forces and it would then be necessary to try to account for these forces, which as explained above, are altogether different from those we should expect to exist. However on measuring the thermo-electromotive force caused by stretching a piece of the iron wire by a weight, it turned out of a smaller order of magnitude than would be necessary for the hypothesis and we are left therefore to the conclusion of the first paragraph above.

In order to establish just what the relation here found is, and to clear up several points with regard to it, it will be necessary to take thermo-electric, change of length, and magnetization curves all from the same specimen in the case of each of the three magnetic metals; and the specimen must be as isotropic as possible.

So far I have only considered junctions at which there is no passage of the lines of magnetization from or to the metal. If the lines came normally from the metal at the junction we should have the other extreme, a case which also remains to be tested.

I hope to continue experiments on the subject.

HAVERFORD COLLEGE,
August 14, 1902.

ON THE THEORY OF THE ELECTROLYTIC RECTIFIER.[1]

By K. E. Guthe.

A POLARIZED electrolytic cell put in an alternating current circuit will always produce a shifting of phase since the current must necessarily become zero when the impressed E.M.F. at the electrodes equals the counter E.M.F. of polarization. The current will lead in phase or, with other words, the electrolytic cell produces the same effect as an electrostatic condenser and we speak therefore of an electrolytic capacity, without however attributing the condenser effects in the two cases to the same cause.

If the counter E.M.F. of polarization on one electrode obtains very different values according to the direction of the current through the cell, then for a given alternating impressed E.M.F. more current will flow in one direction than in the other, or we have what is called an "electrolytic rectifier." A well-known cell, showing this property in a marked degree is the aluminium rectifier, consisting of an aluminium and a carbon (or platinum) electrode, immersed in a solution of a salt of a rather complicated chemical structure, usually alum, $KAl(SO_4)_2$. This cell allows only a very small current to pass from the aluminium to the carbon, while the current in the opposite direction is not obstructed.

To explain this phenomenon two different points of view may be chosen. Either we may consider the obstacle to the current, when the aluminium forms the anode, as a very high ohmic resistance between the liquid and the plate. In fact, a badly conducting whitish film of some oxide or hydroxide of aluminium covers the "formed" plate.

Or we may explain the effect as being due to polarization with a very high counter E.M.F., produced only when the aluminium forms the anode, while it has a value not higher than usual with the

[1] Paper read before the Pittsburg meeting of the A. A. A. S.

current flowing in the opposite direction. On Fig. 1 two curves are plotted showing the relation between the terminal potential difference and the current: curve I., when the aluminium is the anode, curve II., when it is the kathode.

The curves closely resemble those obtained by Le Blanc and others for ordinary polarization, except that the point where curve I.

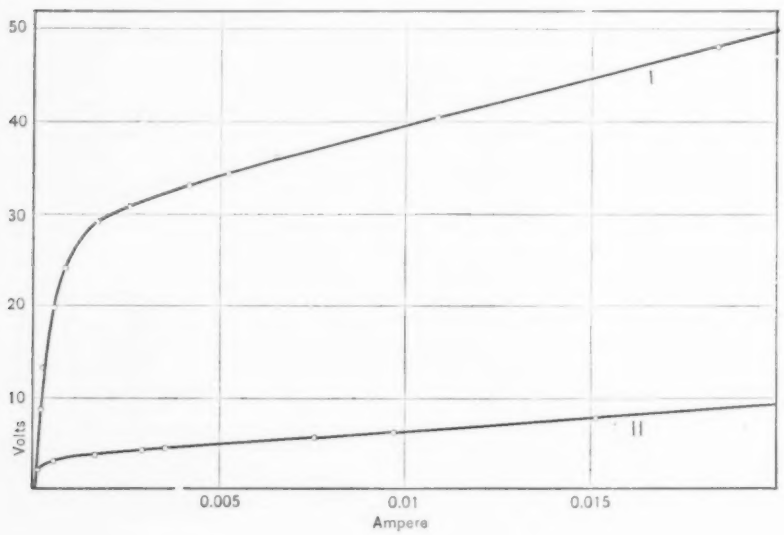

Fig. 1.

bends around corresponds to about thirty volts instead of one or two volts. If we take the point of view first mentioned, we must assume the resistance to be a variable quantity decreasing with an increase of current, *i. e.*, we have to deal with something quite analogous to the "resistance of transition," formerly so often met with in the discussion of ordinary cells. In a former paper[1] I have shown that, at least in the case of copper in copper sulfate solution, polarization will sufficiently account for this apparent, variable resistance.

I prefer, therefore, to look at the problem from the second point of view and state it in the following way: By some cause, whatever it may be, most of the negative ions are prevented from reaching the anode as long as the terminal potential difference remains below

[1] PHYS. REV., 7, p. 193, 1898.

a certain high value, while the kations are not affected more than is the case in ordinary polarization. What is the cause for this?

So far every one experimenting with the aluminium rectifier assumes it to be the layer of oxide, mentioned above. But this seems to me rather improbable. In the case the aluminium forms the anode, we must suppose that with increasing current the layer breaks down or that holes appear in it which close up again, as soon as the current is decreased. We should then expect quite irregular results, but in fact the resistance returns with remarkable regularity to the same value for the same current, no matter whether the current before was larger or smaller. Besides, this return takes place very rapidly, while the changes in resistance are quite slow during the original formation of the film. This contradiction becomes most apparent, when the current is first sent towards the aluminium. The plate acts practically as if no additional resistance besides the electrolytic were present. The oxide film should then be broken up entirely. But as soon as the current is reversed the high resistance forms within a few minutes. Why should the holes close up almost instantly when the formation of the high resistance on a clean plate requires a considerable time? An inspection of the film shows, moreover, that in spite of the various changes of the current it seems to remain unchanged.

If we suppose that the film in question is not the solid oxide film, but of a fluid nature which gives way if the strain becomes too large, but otherwise remains in its position, all the above objections are removed. In order to show that there is such a film on the electrode, I put a cell in which a high resistance aluminium plate was previously formed under the receiver of an air-pump and, when the pressure was sufficiently reduced, could see a stream of very minute gas bubbles escape from the electrode. After this the resistance was quite low, but increased rapidly to its original value as soon as a current was sent through the cell with the aluminium as the anode. When standing for some time, especially after having been in a vacuum, the plate shows a very low resistance, because the gaseous film has been dissolved in the electrolyte.

A point of practical interest is, that after a plate has stood for some time in the solution the film seems to disappear at the surface

line of the liquid, thus lowering the resistance. To get consistent results I had always to raise the plate one or two millimeters before taking a series of observations.

The first result of this investigation is, therefore, that *the high resistance in the aluminium rectifier is due to a thin film of gas, probably oxygen*, which prevents the passage of the negative ions from the solution to the electrode. Only in places where an accidental concentration of energy occurs, the strain may become too great and some ions may break through. This would explain the small current, present even with a low potential difference at the electrodes, the "Reststrom" as it is called in German. If we bring a wire into the liquid and hold it near the electrode, sparks will cross from one to the other and the current increase, to drop again to its low value as soon as the wire is removed.

This theory will also explain the passage of a large current in the direction from the carbon or platinum to the aluminium electrode. Now hydrogen is set free at the latter, but the hydrogen gas is able to escape through the fine meshes of the oxide film in which the oxygen gets so hopelessly entangled. Gas is developed freely at the electrode and there is no high resistance in this case.

One serious objection against the supposition that the resistance is formed by a solid film broken in various places by the current, lies in the fact, that the current hardly changes when we diminish the size of the plate to a very small fraction of the original. Polarization on the other hand would lead us to expect this result.

The following conclusions were drawn from the foregoing considerations and verified by experiment:

1. An increase in temperature will increase the current. The energy of the ions will increase and for a given potential difference, more of them will pass through the gas film. The curves on Fig. 2 show this to be the case. They represent only the observations with the aluminium as anode.

2. The concentration of the electrolyte should have no influence upon the phenomenon. I varied the concentration within wide limits without observing any change in the resistance of the film.

3. The results should not differ appreciably with different electrolytes, as long as similar reactions take place at the anode. I took

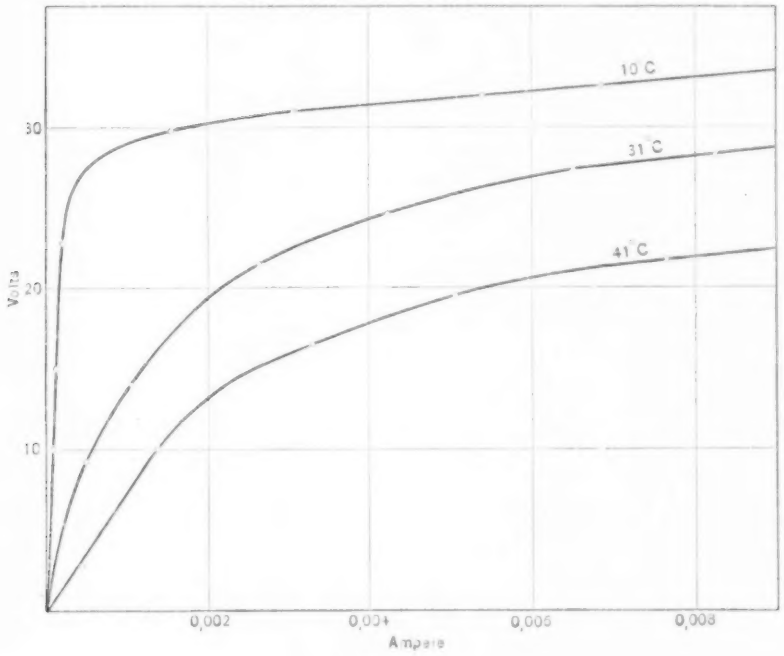

Fig. 2.

two sets of observations, one with solutions of alum (1), ferrocyanide of potassium (2), and zinc sulfate (3) and another with solutions of alum (1′), Rochelle's salt (4) and coppersulfate (5).

The curves are given on Fig. 3 and show that all agree as well with each other as successive tests with the same plate in the same electrolyte would do.

4. The plate may behave quite differently towards other negative ions. A plate showing a high resistance in alum solution offered no resistance when immersed in sodium chloride solution. The chlorine acts as the hydrogen in this respect.

We have then a kind of a semipermeable membrane, which allows certain ions to pass through it and give off their electrical charges at the electrode, while it prevents the passage of other kinds of ions.

It seemed of sufficient interest to study the action of the well-known semipermeable membrane of copper ferrocyanide in this respect. Dr. G. A. Hulett kindly prepared one for me, forming it in a porous cup. The results correspond closely to those obtained

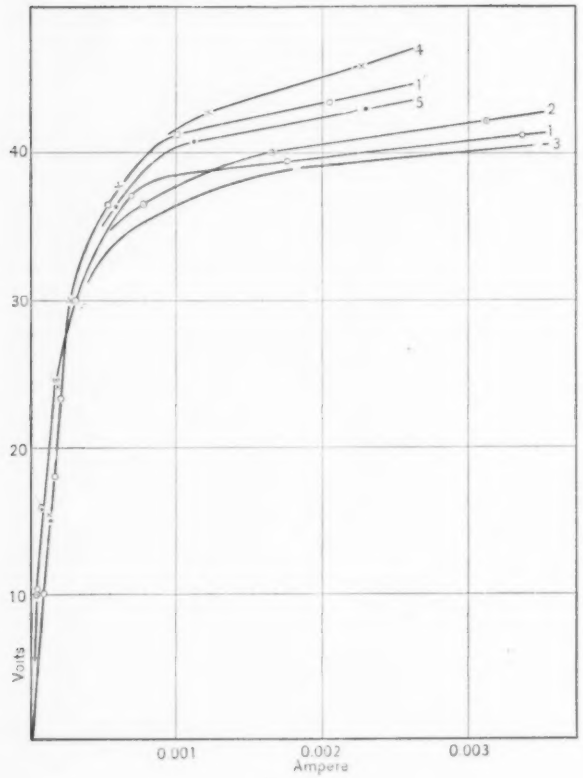

Fig. 3.

with the aluminium cell. Inside the cup was placed a copper electrode immersed in copper sulfate solution, outside a platinum electrode in a solution of potassium ferrocyanide. A high resistance seemed to be present when the current passed from the copper to the platinum, hardly any with the current in the opposite direction. Curves *a* and *a'*, on Fig. 4, show the results obtained with such a cell.

Of course, it does not follow, that the process in this semipermeable membrane is necessarily identical with the one assumed for the aluminium plate covered with an oxide film. Gases seemed, however, to be developed at the porous cup when the current flowed towards the platinum. A further investigation of this subject would be of great interest.

The copper ferrocyanide membrane was also formed by immersing the copper plate directly in the cyanide solution. In this case the hydrogen gas developed between membrane and plate, breaking the membrane off after having collected in sufficient quantity. Otherwise the phenomenon resembles closely that shown by an aluminium electrode. Curves b and b', of Fig. 4, were obtained with such a plate.

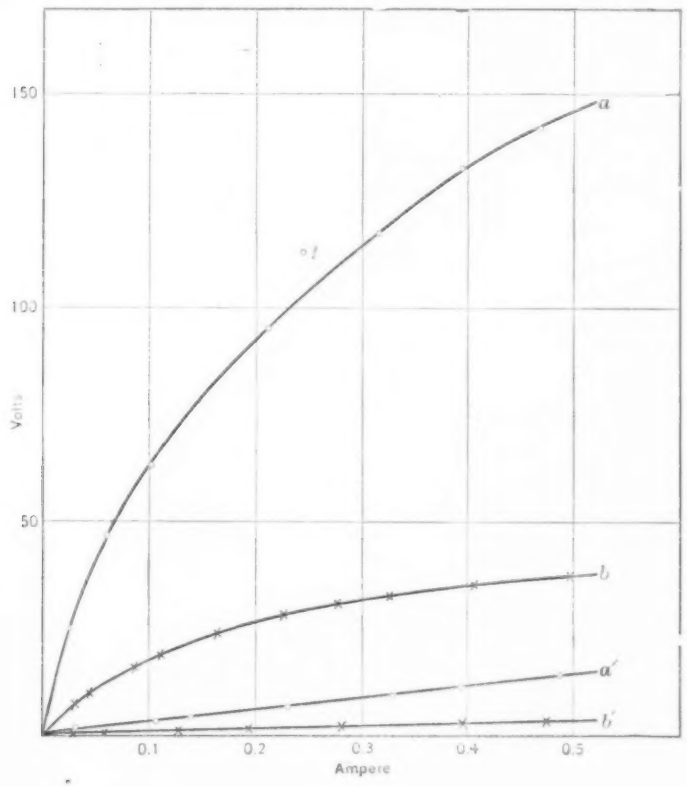

Fig. 4.

The fact, that the polarization falls off very rapidly in the aluminium cell, has been brought forward against the polarization theory. But the polarization is in this case due to the accumulation of ions in the solution only and will, therefore, disappear very rapidly. In ordinary electrolytic polarization there is in addition an occlusion of the ions in the metal of the electrode and this is the cause for the slowness with which it disappears.

In conclusion, we may ask if it may not be possible to explain ordinary polarization as being due to a very thin, non-conducting fluid membrane—possibly pure water as the outermost layer of any solution—which has first to be broken through before the ion reaches the metal. The differences in the value of hydrogen polarization, according as it is developed on platinum, lead or mercury surfaces seem to point to an important influence of the action between the surfaces of the metal and the electrolyte.

Physical Laboratory of the University of Michigan,
August, 1902.

CRYSTALLIZATION FROM A CURRENT-BEARING ELECTROLYTE.

By Paul R. Heyl.

HAVING failed to find any effect produced by electrostatic stress upon crystals produced under its influence (see this Review, February, 1902), the writer has investigated the effect produced upon crystals formed from an electrolyte carrying a current. The results are likewise entirely negative.

Two substances were used: copper sulphate in aqueous solution and mercuric iodide in concentrated hot hydrochloric acid. As in the previous investigation the mercuric iodide was used as a qualitative indicator rather than as a basis for quantitative measurements.

Mercuric Iodide.

The red salt, dissolved in hot concentrated HCl, crystallizes out in the unstable yellow form. An attempt was made to electrolyze this solution between mercury-electrodes in a tube of the form shown in Fig. 1, in the expectation that the liberated iodine would attack the mercury, forming HgI_2, which would redissolve, preserving the strength and clearness of the solution. It was found that large quantities of the insoluble green mercurous iodide were formed, filling the tube and finally stopping the current. Recourse was then had to electrodes of platinum. The iodine liberated at the anode imparts such a deep color to the solution that the whole is soon rendered opaque. To avoid this the arrangement shown in Fig. 2 was used. The three beakers and the siphon tubes contain the so-

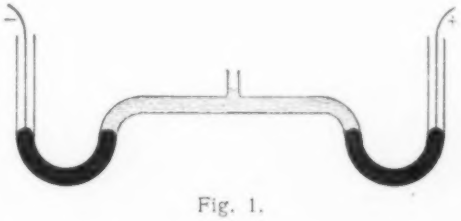

Fig. 1.

lution to be electrolyzed. In the central beaker an additional amount of the salt is dissolved by heat and the current is started. After the crystals had been deposited under the influence of the current they were again dissolved by heat and recrystallized without the current for comparison. The siphon tubes must be large enough to allow a reasonably strong current to pass, and small enough to retard the spread of the iodine-charged liquid around the anode. Those finally used had a bore of 2–3 mm.

Fig. 2.

The greatest current density that could be obtained in the central beaker without contaminating it with iodine was about 0.1 ampère per sq. cm. on an average. Of course it was greater near the openings of the siphon tubes, being in the tubes themselves about twelve times as great. The tubes were much heated, and by a slight increase of the current it was easy to cause steam bubbles to form at the top of the siphon, breaking the current. The crystals in the beaker did not appear to have their formation disturbed at all by the current passing. There appeared some yellow, some orange, all finally turning red. Nor were the points of greatest current density marked by any absence of the unstable yellow form. In no respect did the crystallization with the current differ from that without the current.

COPPER SULPHATE.

In the quantitative study of the angles of the crystals only the three principal dihedrals were noticed; A, the prism angle, B and C, the angles made by the end plane with the side faces. Wherever possible a, b and c, the approximate supplements of these angles, were measured also; sometimes only the angles a, b or c were smooth enough to measure. The angles a, b, c were always entered in the tables as their supplements to facilitate comparison with the others. Thus, where two values of angle A are given for the same crystal they represent A and

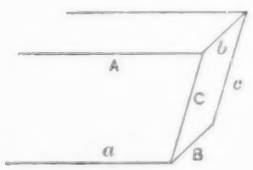

Fig. 3.

$180° - a$; where one value is given it may be sometimes A and sometimes $180° - a$.

The salt used was taken of commercial purity and twice recrystallized. When a current or other supposed cause of disturbance was used it was applied during the second recrystallization. Uniformity of treatment rather than the highest purity was aimed at.

It was soon found that the values of any particular angle would vary about a mean, as did the angles of the sulphur crystals in the previous communication above referred to, and a statistical investigation was seen to be necessary. The object became to accumulate observations of any particular angle until the mean became constant.

Of the 533 angles measured, one fifth were sharp enough to be given on the authority of a single measurement. The others are each the mean of two or more measurements. Each angle was calculated as the measurements were made, and where there was any great departure from the mean the reading could be at once verified.

In calculating the mean of any set of measurements it was decided to give all crystals the same weight. Hence wherever a double entry occurs, the mean of the two values was considered as the contribution of that crystal to the final mean value of the angle in question.

The measurements were made by a spectrometer reading to minutes, except Nos. 26–58 of lot 1, and Nos. 25–59 of lot 2, which, by the courtesy of Dr. A. W. Goodspeed, were measured on an instrument of higher grade, a Fuess goniometer reading to half minutes, at the Randal Morgan Laboratory of the University of Pennsylvania. The definition of the angles was as a rule too poor to allow of readings being taken more closely than a single minute.

Lot 1 was made from a hot-water solution allowed to stand over night.

The final means in this lot are probably trustworthy to a single minute. In the case of angle A, where the variability is greatest, the apparently large fluctuation in the last seven means is due to a small variation above and below the value 64.5.

Lot 1. *Without current. Time,* 18 *hours.*

No.	Angle *A.*	Mean.	Angle *B.*	Mean.	Angle *C.*	Mean
1	123° 10′	122° 70′	108° 17′	108° 17′		
2	123 34 123 7	75	108 15 108 31	20		
3	122 52 123 23	73				
4	123 10 123 10	72	108 1 108 1	14		
5	122 56 123 25	72	107 48 108 5	9		
6	122 53 123 13	70				
7	122 51 123 39	71				
8	123 29 123 22	73	108 10 107 58	8	127°32′127°34′	127° 33′
9	122 57 123 11	72	108 13 108 0	8		
10	123 10 123 0	71	108 31 108 3	9		
11			107 55 107 54	7	127 38 127 35	35
12			108 9 107 59	7	127 28 127 33	34
13			107 51 108 2	6	127 31 127 31	33
14			107 50 108 0	5	127 38 127 35	34
15			108 2 108 1	5		
16			107 53 107 47	3	127 38 127 32	34
17			108 4 108 10	4		
18			107 51 107 45	3		
19			107 56 107 55	2	127 30 127 28	33
20	122 52 122 51	70				
21	122 48 123 2	68	107 49 107 48	1		
22	123 13 122 54	68	108 11 107 59	1	127 42 127 34	34
23	122 50 123 20	68	107 49 107 47	1	127 32 127 33	33
24	122 59 122 55	67	108 4 107 55	1		
25	123 27 123 7	68	108 5 108 11	1	127 30	33
26	123 20 122 59	68	108 13 107 44	1	127 29 127 39	33
27	123 4	67	108 38	3	127 37	34
28			107 21	1	127 39	34
29	123 26 123 6	68	108 18 107 52	1	127 29	34
30	122 44 122 33	67	107 35 107 56	0	127 31	33
31	123 8	67	107 52 108 2	0	127 32	33
32	123 12 122 55	66	108 16 108 1	1	127 32	33
33	123 2 123 3	66	108 8 108 24	1	127 37	33
34	122 44	65	107 50	1	127 13	32
35			107 57	1	127 24	32
36	123 20	66	108 22 107 53	1	127 47 127 26	32
37	123 16 123 14	66	108 9	1	127 22	32
38	122 47 122 56	66	107 56	1	127 22	31
39	122 43 122 55	65	107 53	1	127 28	31
40	122 39	64				
41	123 1	64	107 55	1	127 42	32
42	122 50	64			127 29	32
43			108 4	1		
44			108 15	1	127 31	31
45	123 23	64	108 20	1	127 34	32

Lot 1.—*Continued. Without current. Time,* 18 *hours.*

No.	Angle *A*.	Mean.	Angle *B*.	Mean.	Angle *C*.	Mean.
46	123 35 122 46	64	107 53	1	127 41	32
47	122 48 122 59	64	107 39	1	127 31	32
48	122 34 122 34	63	108 16	1	127 38	32
49	122 55	63				
50	123 38	64	108 8	1	127 30	32
51	123 23	64	108 17	2		
52	123 22	65	108 4	2	127 32	32
53	122 57	65	108 0 108 8	2		
54	122 42 122 50	64	107 52 107 57	2		
55	123 37 122 51	65	108 18	2		
56	122 51	64	107 52	2		
57	123 16	64				
58			108 41 108 24	2		
Mean of 44, 123° 4′.			Mean of 50, 108° 2′.		Mean of 33, 127° 32′.	
Average departure from mean, 25′.			Average departure, 11′.		Average departure, 5′.	

The second lot was made from a hot-water solution under the influence of a current. A shallow, flat-bottomed glass dish, about 15 cm. in diameter, contained the solution. The current was caused to enter at the middle of the dish and passed out by a strip of sheet brass encircling the inside of the wall of the dish, thus passing radially through the solution, the current density being greatest, of course, in the center. For the central anode copper was tried at first with a view to preventing the agitation of the solution by gas bubbles and keeping up its strength. This was not a success, as a good deal of sludge formed at the anode and spread through the solution. The copper anode was then put in a small porous pot in the center. This eliminated the sludge trouble, but so confined the heat generated that the contents of the pot would boil dry in less than an hour. It was finally decided to use a platinum anode. A platinum crucible, filled with scraps of copper to weight it, placed in the center of the dish, afforded a satisfactory entrance for the current. The average diameter of the immersed part was about 1.5 cm. It was found that the oxygen bubbles rose quietly close to the crucible without troubling the solution more than 1 cm. away. It was recognized that by the use of a platinum anode free sulphuric acid must accumulate in the solution, but the effect of this was put aside for later investigation.

Lot 2. *Current density* = 0.07 *ampère per sq. cm. (average). Time*, 4 *hours* 45 *minutes.*

No.	Angle *A.*	Mean.	Angle *B.*	Mean.	Angle *C.*	Mean
1	122°42′	122°42′				
2	123 13	58				
3	123 6 123 7	61	107°48′	107°48′		
4	122 53 122 42	57	107 55 107 54	51	127°35′ 127°22′	127°29′
5	122 47 122 46	55				
6	123 17 123 13	58	107 59 108 3	55	127 29	29
7	123 9 123 9	60	107 58 107 54	55	127 31 127 30	30
8	123 17 123 13	62	107 58 107 58	56	127 39 127 42	32
9	122 53 122 59	61	107 55 108 18	57	127 15 127 15	29
10	122 53 122 53	60	107 50 107 46	56	127 44 127 41	31
11	122 58	60				
12	123 0 123 2	60	107 43 107 46	55		
13	123 0 123 8	61	108 5	56	127 20 127 26	30
14	122 59	60	107 56	56	127 30	30
15	122 49 122 51	60	107 58 108 2	56	127 32 127 36	30
16	123 13	61	108 0	56	127 24	30
17			108 29 107 54	58	127 24 127 32	30
18			108 8 108 1	58		
19			107 50	58	127 31	30
20			107 48 107 45	57	127 14	28
21			108 14 108 16	58	127 25	28
22			107 37 107 50	57	127 44	29
23	122 48	60			127 28	29
24			108 8 108 8	58		
25			108 9	58		
26	122 45	59	108 14	59	127 34	29
27	123 0	59	108 17	60		
28	122 51	59	108 4	60	127 28	29
29	122 52	58	107 43	59	127 21	29
30	122 48	58	107 53	59	127 32 127 33	29
31	122 51 122 52	58	108 11	60	127 42	30
32	121 54 122 2	58				
33	122 44 122 48	57	108 11	60	127 23	30
34	122 52	57				
35	122 59	57				
36	123 12	58				
37	123 15 122 57	58				
38	123 6	58				
39			107 47 107 46	60	127 28	29
40	122 43	58	108 52	61	127 34	30
41	123 1	58	108 22	62	127 28	30
42	122 58	58	107 56	62	127 25	29
43			108 54	63		
44	123 11 123 18	58				

Lot 2.—*Continued. Current density* = 0.07 *ampère per sq. cm. (average). Time,* 4 *hours* 45 *minutes.*

No.	Angle *A*.	Mean.	Angle *B*.	Mean.	Angle *C*.	Mean
45	122°40′ 123 2	58				
46			108°19′	64		
47			108 17	64		
48	123 11 123 56	58	108 4	64	127°27′	29
49	122 57 122 57	58	108 1 108 5	64	127 27	29
50			108 6	64		
51	123 1	58				
52	122 47	58	107 51	64	127 28	29
53	123 1	58	107 57	64		
54	123 9	58	108 2	64	127 8	28
55	123 2 123 5	58	107 56	64	127 47 127 59	29
56	122 48	58	108 17	64	127 56	30
57	122 52	58	108 19	64	127 30	30
58	122 48	58				
59			107 52	64	127 1 127 19	29
Mean of 45, 122° 58′. Average departure, 8′.			Mean of 44, 108° 4′. Average departure, 12′.		Mean of 34, 127° 29′. Average departure, 7′.	

The total current passing through the solution was 2.4 ampères; the average current density 0.07 ampère per sq. cm. of cross section of fluid; the time 4 hours and 45 minutes; the volume of solution, about 200 c.c. At the expiration of the above time the solution was still warm to the touch. This was evidently due to the heating effect of the current, as the first lot was made in the same dish under the same conditions, and cooled much more rapidly. The possible effect of the heating during the whole period of growth of the crystals was also reserved for future investigation.

It is possible, though not probable, that the small differences shown by the final means of angles *B* and *C* in lots 1 and 2 are due to an insufficient number of observations; but such an explanation is hardly possible for the 6′ difference in the final means of angle *A*. Attention is called to the fact that the mean of *A* in lot 1 is not altered more than 1′ in the last 17 crystals, while the corresponding mean in lot 2 is not altered at all in the last 18; and any future variation of the mean must be expected to proceed at an increasingly slow rate. There are three possible factors that may have produced this change — the current, the accumulation of free sul-

phuric acid, the heating effect. Each of these three possibilities was tested in turn.

The third lot was made with the same apparatus as the second lot, but with a greater current density. The total current was 5.7 ampères; the average current density 0.23 ampère per sq. cm.; the volume of solution about 150 c.c. There was a strong development of heat and oxygen at the anode, and in 20 minutes the solution in its immediate vicinity had turned to a solid cake of copper sulphate, the moisture next the crucible was either evaporated or electrolyzed away and the current stopped. The rest of the solution contained small crystals, none over 2 mm. long, very imperfect about the ends, but with the angle *A* in fair condition. As this was the angle of which statistics were most needed, no especial effort was made to find measurements of the other angles.

Lot 3. *Average current density* = 0.23 *ampère per sq. cm. Time*, 20 *minutes.*

No.	Angle *A*.	Mean.	No.	Angle *A*.	Mean.
1	122°46′	122° 46′	23	122°55′	56′
2	122 59	53	24	122 48	56
3	122 49 122 45	51	25	123 13	56
4	122 54	52	26	122 45 122 44	56
5	122 53	52	27	123 1	56
6	122 49 122 55	52	28	122 27 123 0	56
7	122 49 122 55	52	29	122 52	56
8	123 14 122 55	53	30	123 21 123 3	56
9	123 17 123 9	56	31	123 14 123 3	57
10	122 48	55	32	122 51 122 57	56
11	122 51	55	33	123 11	57
12	122 46 122 58	54	34	123 10	57
13	123 11 123 9	56	35	123 13	58
14	123 8 122 45	57	36	122 51	58
15	122 56 123 2	56	37	123 11 122 45	58
16	122 46	55	38	122 58	58
17	123 3 123 5	56	39	122 47 122 49	57
18	122 47	55	40	122 53	57
19	123 4	56	41	122 53	57
20	122 52	56	42	123 2	57
21	123 8 123 7	56	43	122 57	57
22	122 56	56			

Mean of 43, 122° 57′. Average departure, 8′.

The close agreement of the final mean of this lot and the preceding one coupled with the fact that each mean is probably accurate

to a single minute leads us to conclude that the variation in the angle is independent of the current strength.

To test the influence of the accumulated sulphuric acid a fourth lot was made from a solvent consisting of 90 per cent. volume of water and 10 per cent. volume of strong H_2SO_4. It may be calculated from the data already given that when the current was stopped lot 2 contained about 20.5 gr. H_2SO_4, occupying about 11 c.c.; hence the per cent. by volume was about 5 or 6. Lot 3 in like manner contained about 3.4 gr. or 2 c.c. H_2SO_4, less than two per cent. by volume. The percentage of acid in the solvent used was therefore greater than that present at the end in either the first or second lot.

The hot solution was allowed to stand for 18 hours, as with lot 1. The angle *A* was the only one measured.

Lot 4. *From dilute* H_2SO_4, 10 *per cent. volume. Time, over night.*

No	Angle *A*.	Mean	No	Angle *A*.	Mean
1	123°25′	122° 85′	23	123° 32′	122° 64′
2	122 36 122 47	64	24	122 46 122 42	63
3	123 16 123 18	68	25	122 52 122 59	62
4	122 58 122 53	64	26	122 42	62
5	123 3 123 29	67	27	123 10 122 58	62
6	123 33 123 14	70	28	123 31	63
7	123 3 122 43	68	29	123 7	63
8	123 5	67	30	122 47 122 49	63
9	123 14 123 17	68	31	123 46 123 32	64
10	123 3	68	32	122 53 122 51	64
11	123 6 123 5	68	33	123 6	64
12	122 58	67	34	122 40 123 1	63
13	123 5 123 18	67	35	122 51	63
14	122 55 122 56	66	36	122 52 123 1	63
15	123 4	66	37	122 55 123 1	63
16	122 41	65	38	123 39	64
17	122 57 122 58	64	39	123 14	64
18	123 3	64	40	123 17	64
19	122 30	62	41	123 0 123 18	64
20	123 3	62	42	122 56	64
21	123 7	63	43	122 58 123 0	64
22	123 7	63	44	122 53	64

Mean of 44, 123° 4′. Average departure, 12′.

From the agreement of the final mean of this lot with the corresponding mean of lot 1 we are entitled to conclude that the presence

of sulphuric acid in the solution exercises no influence over the crystal angle.

To test the effect of heat during the whole growth of the crystal a fifth lot was made from hot water. After half an hour, when the solution was still warm to the touch, the crystallization was stopped.

Lot 5. *Time*, 30 *minutes.*

No.	Angle *A*.	Mean.	No.	Angle *A*.	Mean.
1	122°53′ 122°57′	122°55′	11	123°11′ 122°58′	122°52′
2	122 46 122 42	50	12	123 2 123 3	53
3	122 58	52	13	122 50 122 56	53
4	122 48 122 45	51	14	122 41 122 50	52
5	122 51 122 55	51	15	122 53 122 53	52
6	122 51	51	16	122 52 122 55	52
7	122 48 122 52	51	17	122 49	52
8	122 50 122 52	51	18	123 0	53
9	122 46 122 32	50	19	122 55 122 59	53
10	122 57	51	20	122 58 123 0	53

Mean of 20, 122°53′. Average departure, 5′.

It was not necessary to carry the measurement of this lot very far to arrive at the explanation of the variation of the prism angle. By referring to the other tables it will be seen that even with the angle that showed the greatest variability (*A* of lot 1) the mean did not change more than 3′ after twenty crystals had been measured; while with angle *c* lot 1, where the average departure is the same as that of lot 5, the 20-crystal mean and the final mean are the same.

As stated at the beginning, the conclusion is negative; that the passage of the current produces no effect comparable with that produced by the heating of the solution.

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ELECTRICAL DISCHARGE FROM HOT CARBON.

By C. D. Child.

THE present article is a continuation of a series of articles which have been published on the electrical discharge through gases caused by bodies at high temperatures. The first one of these gave an explanation of the electric arc. In that article it was assumed that the positive ions in the arc move more rapidly than the negative ones. In the next article it was shown that when ions are drawn out from the arc by a charged body, the positive do indeed have the greater velocity.

In a more recent article an account was given of the discharge from a hot platinum wire. Here also the positive ions under some of the conditions move more rapidly than the negative ones. In this last case it was possible to take one step further, and to give a probable explanation of the greater velocity of the positive ions. Apparently the average velocity of the ions was greatly diminished by the presence about the wire of particles driven off from it, and these particles retarded the movement of the negative ions more than the positive. Some of the particles driven off were either charged negatively from the beginning or else quickly became thus charged. In any case more of them seemed to act as slowly moving negative ions than as positive, and the average velocity of the negative ions was thus made smaller than that of the positive. This explanation was suggested as a possible one by Rutherford[1] before my article on the same subject was completed.

This phenomenon is not as unique as might at first be thought. Rutherford[2] found that "emanation" from the thorium compounds behaved as if charged positively. Barus[3] has reached the conclusion from his work on the diffusion of nuclei that in some cases they may exist in a colloidal state when in a gas. If the platinum

[1] Phys. Rev., 13, 332.

[2] Phil. Mag. (5), 49, 161.

[3] Amer. Journ. of Science (4), 13, 400.

particles driven off from the hot wire are in such a condition we may very properly compare them with the platinum particles which Bredig[1] secured in the colloidal condition when an arc is formed between platinum terminals under water. He found that when a current was passed through such a solution these particles are carried to the anode, as if they were charged negatively, as are indeed many of the particles driven from the hot platinum wire. He also found the same phenomena to exist in the case of other metals. Possibly the colloidal condition of particles when in a gas may be found to be of more common occurrence than has been supposed.

With this knowledge of the ions passing from platinum it seemed most natural to suppose that the greater velocity of the ions drawn from the arc and also those in the arc itself was due to the same cause, namely to particles driven off from the carbon which were either charged negatively from the beginning or became so when surrounded by negative ions. I have, therefore, repeated some of the experiments, using a pencil of carbon instead of platinum wire.

It was not expected that the phenomena would be identical in the two cases, since the particles of carbon which may come in contact with oxygen while they are yet hot will form CO_2 or CO, and since the electrical properties of carbon are no doubt different from those of platinum.

Some of the experiments could not well be repeated with carbon, since the carbon pencil in air commences to burn as soon as it is hot enough to produce discharge, and this decreases the diameter of the pencil. Thus the same amount of current through the carbon will continually heat it to higher and higher temperatures. Therefore no attempt was made to repeat any but the simpler experiments which might have some bearing on the explanation which has been suggested.

These experiments will show that, though the details are in many respects different, the general phenomena are the same with carbon as with platinum, and they make probable the explanation that the positive ions move the more rapidly because the negative ones are loaded with particles driven off from the carbon.

Discharge with Different Currents through the Carbon.—The first

[1] Ztschr. angew. Chem. Jahrgang, 1898, Heft 41, p. 952, Beib. 24, 229.

measurements were those showing the effects produced by varying the current through the carbon, when it was heated in the air. These measurements must of necessity be more or less inaccurate. The carbon not only becomes smaller the longer it is heated, but the temperature is affected both by the amount of current flowing through it and by the amount of combustion at the surface. Still by taking measurements as quickly as possible and by beginning with small currents some idea can be gained of the way in which the rate of discharge depends on the temperature.

The observations given in the following four tables were all taken with the same cylinder about the carbon. The carbon pencil used with the first set was 3.1 mm. in diameter. This gradually became smaller as it was burnt away. In the other three cases the observations were made in a glass cylinder which has already been described[1] and in these cases the carbon pencil was 1.6 mm. in diameter. The diameter of the surrounding cylinder was 4 cm. and

TABLE I.

Current through Pencil.	Positive Discharge	Negative Discharge.
8.5	7.3×10^{-9}	
9	42	
9.5	29	29×10^{-9}
10	17	19
11	12	13
12	6.8	7.8

the length 5 cm. In each of the tables column 1 gives the current in ampères flowing through the carbon pencil. Column 2 gives the rates of discharge in ampères when the carbon was charged positively, and column 3 those when it was charged negatively. The observations were made in the same way and with the same instruments that have been described in connection with measurements on the discharge from platinum wires.

Table I. gives the observations taken with the carbon in air. The potential difference between the carbon and the surrounding cylinder was in this case 35 volts.

[1] PHYS. REV., 14, 242.

After taking the data as given above the current was diminished somewhat and the discharge was found to increase, showing that the smaller rates of discharge were indeed due to higher temperatures and not to the wasting away of the carbon pencil.

As seen in the table the positive discharge commences before the negative. It was, however, difficult to compare these with certainty, since combustion began at nearly the same temperature as that at which discharge commenced and with the beginning of combustion there was a rapid rise in temperature. To test this further a device which has already been described[1] was used for reversing the direction of the current about once per second. By these means the surrounding cylinder receives first positive and then negative discharge and the average potential would indicate which of these was the larger. When this was used it was found that the cylinder became charged positively as the carbon first began to glow. But when the amount of current sent through was increased the average potential of the cylinder became a few volts below zero. This fully confirms the data given in the table. The positive discharge first occurs, but the negative becomes the larger at higher temperatures.

When we compare this with the discharge from platinum wires, we see that in both cases the positive discharge first occurs and that in both cases both the positive and negative discharges pass through maxima. The two cases are different in that with the platinum the negative discharge requires much higher temperatures than the positive, while with the carbon only a little higher temperature appears to be needed. They are also different in that with the carbon the negative soon becomes larger than the positive. The fact that the positive and negative begin with nearly the same amount of current is partly due to the combustion which begins at this point. The other differences will be considered later.

The rate of discharge was found to vary approximately as the square of the potential difference.

Discharge within an Enclosed Tube. — When observations were made of the rate of discharge within an enclosed tube it was found that the phenomena were the same as those observed under similar

[1] PHYS., REV., 12, 139.

circumstances with platinum. The rates of discharge with different currents are given in Table II.

TABLE II.

Current through Pencil.	Positive Discharge.	Negative Discharge.
4	9×10^{-10}	
4.5	6	
5	2.4	
7	1.2	3.6×10^{-10}
8	1.3	6
10	1.1	6
11	1	4

The rates of discharge are here very much smaller than in open space and show that here also particles are driven off which collect on the ions and retard their motion.

Discharge in a Vacuum. — When we pass to the discharge in a

TABLE III.

Current through Pencil.	Positive Discharge.	Negative Discharge.
4.5	3.9×10^{-7}	
5	12	
6	26.7	
8	33	
9	40	1.9×10^{-7}
10	41	10
11	45	45.5
12	43	143
13	47.5	510
14	60	1370

vacuum we find an even more complete parallel with the phenomena with a platinum wire. The data are given in Table III. for the discharge at approximately 1 mm. of mercury pressure.

With larger currents the tube and the connections became hot so quickly that it was not practicable to carry the observations further. To judge from the appearance of the carbon it had with the largest current about the same temperature as the filament of the ordinary incandescent lamp when used at its normal candle power.

As with platinum wire the positive discharge first begins to pass from the carbon, but after the negative once starts it quickly sur-

passes the positive. The negative discharge becomes very much larger with the carbon than it did with the platinum, possibly because higher temperatures may be reached. The phenomenon here is essentially that known as the Edison effect. It has been shown by Edison, Preece,[1] and Fleming[2] that in an ordinary incandescent lamp a current will flow from the negative side of the filament to an electrode within the lamp. This was found by Fleming to amount to several milliampères in many cases. Fleming also found that a very small current flows from the positive terminal. There seemed at first sight some discrepancy between the much greater negative discharge from carbon in the lamp and the greater readiness of the positive discharge to pass from the carbon in air at normal pressures. But these observations show that the differences in behavior are due to the varying conditions of the gas about the carbon and to the temperature.

It has been suggested that the discharge in the lamp was carried by particles of carbon. This would scarcely seem to be a complete explanation. We have reason for supposing that the particles of carbon diminish rather than increase the amount of discharge from carbon in gases at atmospheric pressures, and the same no doubt is true here. The explanation offered by Merritt and Stewart[3] seems the more probable one.

Discharge in Hydrogen.—Observations were also made with hot carbon in hydrogen gas. The data taken are given in Table IV. The potential difference was 25 volts.

The positive discharge first shows itself as in the corresponding case with platinum, and the discharge with both platinum and carbon is much larger in hydrogen than in the enclosed tube with air or with a mixture of nitrogen and CO_2 as the case may be. With carbon in this mixture the space became smoky, but in hydrogen it remained clear. Neither platinum nor carbon disintegrate in hydrogen as in other gases. But in other respects the two cases are not the same. With the platinum there was no evidence that the positive discharge passed through a maximum, while here there is plain

[1] Proc. Roy. Soc., 1885, 219.
[2] Phil. Mag. (5), 42, 52.
[3] Science, 15, 427.

evidence of such action. With the platinum the negative discharge began soon after the positive, but with carbon it took nearly twice

TABLE IV.

Current Through Carbon.	Positive Discharge.	Negative Discharge.
5.5	2.5×10^{-9}	
6	11	
7	15	
8	12.5	
9	11	5.8×10^{-9}
10	9	11
11	9.5	9.5
12	7.5	7

as much current to produce the negative discharge as to produce the positive. Moreover, with the platinum the negative discharge becomes very much larger than the positive, while here the negative scarcely exceeds the positive.

These last two phenomena are somewhat surprising, since under all the other conditions which have been tried the ratio of the negative discharge to the positive was larger with the carbon than with the platinum, while here it is smaller. I have at present no explanation for this fact.

Let us see if any conclusions can be drawn from the work thus far described. The work was undertaken to determine, if possible, two questions: first, are particles of carbon given off which diminish the velocity of the ions? and second, do these particles diminish the velocity of the negative ions more than that of the positive? In regard to the first question there can be no doubt. The particles certainly retard the velocity. At the higher temperatures where more particles are driven off the rate of discharge becomes smaller. When the carbon is enclosed in a tube, it becomes very much smaller and in this case the discharge is greater when the carbon is first heated than it is after a few seconds when the particles have become more numerous in the tube.

But when we come to the second question the evidence is not so clear. The positive discharge is able in every case to pass from the carbon at lower temperatures than the negative. This indicates that there is greater attraction between the carbon and the negative

electricity than between the carbon and the positive. From this we should expect the particles to load the negative ions to a greater extent than the positive.

On the other hand in the air and within the tube with the gases at atmospheric pressure the negative discharge is greater than the positive. This would indicate a greater velocity on the part of the negative ions. In a vacuum the negative discharge becomes very much larger than the positive. This last, however, is probably due to a secondary ionization produced by the collisions between the ions and the molecules. It may be that when the carbon is in a gas at normal pressure there is still a slight tendency for the particles to collect on the negative ions to a greater extent than on the positive, but that this tendency is not sufficient to overcome the greater velocity which the negative ions ordinarily have. Moreover, it must be remembered that when the carbon is in the air many of the particles will be oxidized and that when it is enclosed in a tube it is not surrounded by air but by a mixture of nitrogen and CO_2.

Of these various facts none would seem to cast more light on the subject than the fact that it requires a higher temperature to cause the negative electricity to escape than to cause the positive to do so. From this we should expect the negative ions to be loaded more than the positive. Evidently we can not make any definite statement as yet concerning this matter, but it is equally evident that there is no reason for denying the possibility of a greater loading of the negative ions under certain conditions, if such a supposition is needed to explain the phenomena of the electric arc.

We may now take a further step and offer an explanation of the more rapid motion of the positive ions in the arc by assuming that the negative ions are there loaded with particles of carbon. The temperature of the arc must be very near to the boiling point of the carbon. There must be some parts of the arc that are cooler than others. If then the carbon is vaporized in some parts it must be condensed in others and it is reasonable to suppose that these particles should become charged negatively, just as the particles of platinum from the vapor of platinum formed in the arc under water become thus charged. That is the arc consists of hot vapors and

of carbon particles in the colloidal state which carry negative charges. This is in harmony with all known facts and furnishes an explanation upon which further experiment may throw more light.

Summary.—When a carbon pencil is heated the positive discharge occurs first. This is true whether the carbon is heated in air at normal pressures, in a vacuum, or in hydrogen. In air it requires only a slight increase in temperature to produce the negative discharge. At higher temperatures the negative discharge is the greater. Both positive and negative pass through maxima as the temperature is raised.

Within an enclosed tube the discharge is very much smaller than in open air.

In a vacuum the negative discharge becomes many times greater than the positive at higher temperatures. The rates of discharge do not here pass through maxima.

In hydrogen at higher temperatures the positive and negative discharge have nearly the same value. The discharge is here much higher than in air. Both discharges pass through maxima.

In general the discharge from hot carbon behaves in a similar way to that from hot platinum. The velocity of the ions is diminished because of the presence of particles driven off from the carbon. The smaller velocity of the negative ions in the electric arc is quite probably due to the loading of the negative ions with these particles.

Since writing the above my attention has been called to the work by Stenger on the arc in a vacuum, and its relation to the data given above in Table III. It appeared from that table that at higher temperatures the negative ions move more rapidly than the positive. Even if we suppose the negative discharge is largely due to secondary ionization, it would still be necessary to suppose a greater velocity of the negative ions in order to produce this effect. Of course the conditions in the arc are quite different from those which we have been studying, and yet it is not unreasonable to expect that the negative ions will also have the greater velocity in the arc when in a vacuum and consequently a greater fall of potential at the negative carbon.

As far as I know no one has investigated this point, but Stenger[1]

[1] Wied. Ann., 25, 31.

has observed the intensity of light at the ends of the arc when in a vacuum. He found much trouble in maintaining the vacuum after the arc was started, but he was at length able to obtain one of 1 or 2 mm. of mercury pressure. He then found the intensity of light at the two carbons to be about the same. Now the intensity of light is probably greater where the fall of potential is greater, so that in the case examined by Stenger the fall of potential at the two carbons were probably the same.

I have myself endeavored to measure the fall of potential in the arc when it was in as high a vacuum as possible, but have not been altogether successful. Besides the difficulties of obtaining a high vacuum there was the added one of placing the exploring carbon at the desired point in the arc. I was not able to do this with any apparatus which gave even as high a vacuum as that obtained by Stenger. As a result the data obtained were very irregular, so that no more could be said than that the falls of potential at the ends were much more nearly equal when the arc was in a vacuum of 2 or 3 mm. pressure than in air at normal pressures. But even with this we see that there are reasons for believing that the change which we would expect would really occur, if we could secure a vacuum sufficiently high.

COLGATE UNIVERSITY, Sept., 1902.

CARBON RESISTANCES.

By A. C. Longden.

ALTHOUGH our dependence upon carbon as a high resistance material is not as nearly absolute as it was a few years ago, yet I suppose it is true to-day that the majority of electrical resistances ranging from 1,000,000 to 100,000,000 ohms and used for general experimental purposes are made of carbon. Perhaps the most common form is the graphite line on a ground-glass surface.

Of course carbon resistances at best are very much inferior to wire resistances, but when a number of resistances of several megohms each are required, the majority of us have to look for something cheaper than wire. The use of a less expensive substitute is not a matter of choice, but a necessity.

A few years ago, while looking for a satisfactory form of carbon for a resistance material, I hit upon the idea of carbonizing sugar in clay sticks. In April, 1897, I mixed 10 grams of pipe clay and 2 grams of flake graphite with 10 cubic centimeters of a 20-per-cent. solution of ordinary granulated sugar. The moist clay was then pressed into cylindrical sticks about 5 millimeters in diameter. These sticks were thoroughly dried in an oven at 150 degrees and then carbonized by keeping them at a red heat 8 hours in a covered crucible, packed with graphite.

I then prepared another series of carbon sticks in every way similar to those just described, except that the graphite was omitted, so that the resulting sticks contained no carbon except that which was furnished by the sugar. In both cases the results were tolerably satisfactory, although there was no great uniformity in either. The resistance of the carbon sticks in the first series was from 1,000 to 5,000 ohms per centimeter, while that of the second series was several megohms per centimeter.

Another lot, similar to the first (with graphite), but more thoroughly carbonized (20 hours) varied in resistance from 25 to 200

ohms per centimeter; and still another lot, carbonized 3 hours only, varied from 5,000 to 40,000 ohms per centimeter. Of course, even if the carbonization were thorough in every case, the resistance of these sticks may be varied indefinitely by adjusting the amounts of graphite and sugar in the mixture before carbonization.

One of the objections to these resistances is the rapid increase of resistance with time. This increase is represented graphically by the curves in Fig. 1, where time is plotted on the X-axis and resistance on the Y-axis. These curves are very fair representatives of

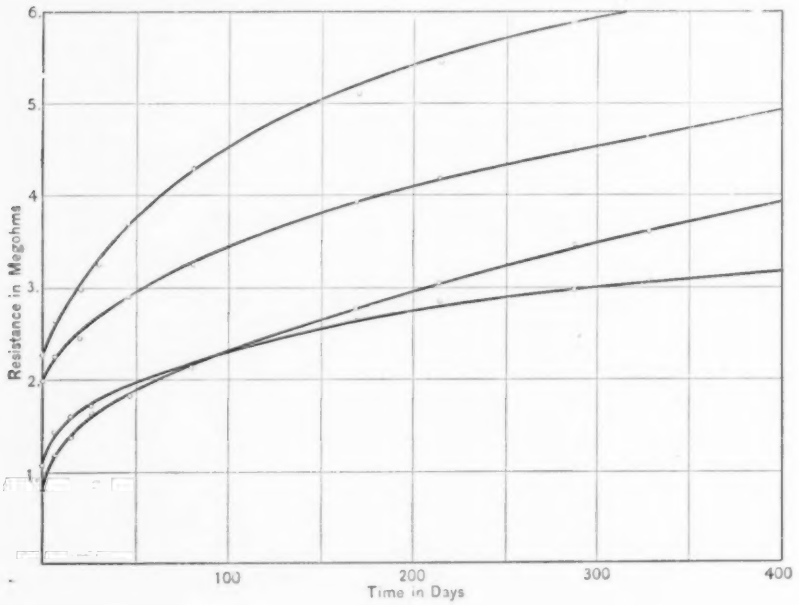


Fig. 1.

the class of resistances described above. It is to be noted that while the resistance increases very rapidly at first, it is also true that after a sufficient length of time the change is so slow as to be quite negligible during a brief interval, such as a few days, or even a few weeks.

If no other objection could be urged against these resistances, they might be used in any short series of experiments, and, if standardized immediately before and after the series of experiments, their values *during* the experiments might be known to a degree of accuracy which would satisfy the requirements of all ordinary work. An-

other, and a more serious objection, however, demands our consideration. It is the unreliable character of the junctions between the carbon sticks and their terminal wires. The resistance of the junction should be small and constant. As a matter of fact, if the wires are fastened on with clamps, the resistance of the joint is usually high and quite variable. The variations are sometimes so great as to indicate that there is more resistance in the joints than anywhere else. For example, on one occasion a resistance fell from 1,840,000 to 746,000 ohms upon tightening the clamps, and, upon still further increasing the pressure, the resistance fell to 357,000 ohms.

Doubtless the variations which occur after the clamps are permanently set are largely due to the difference in coefficient of expansion between the carbon and the clamp. At any rate, temperature changes frequently produce changes in resistance which can hardly be accounted for on any other hypothesis.

In support of this view I cite another example. In this case the terminal wires were twisted around the ends of the carbon stick and then a mass of melted lead was cast around the joint. The lead of course answers the purpose of a closely fitting clamp, binding upon all sides of the carbon stick; but as the lead and the carbon have different coefficients of expansion, the pressure of the clamp is regulated by the temperature. The effect upon the resistance of the joint is shown in Table I., which displays the results of a number of resistance measurements made upon the same carbon stick at different temperatures.

TABLE I.

Resistance in Ohms.	Dif.	Temperature.	Dif.	Change in Resistance per Degree.
43495		14.1°		
	1465		2.1	698
42030		16.2		
	910		1.3	700
41120		17.5		
	499		.7	713
40621		18.2		
	2189		3.1	706
42810		15.1		

From the results in the last column it will be seen that the resistance changes followed the temperature changes within two per cent., which is quite as close agreement as the thermometer readings would warrant. Of course the experiment suggests temperature

coefficient; but the magnitude of the change in resistance per degree is very much greater than the temperature coefficient of the material would explain. I am inclined to believe that the contact resistance between a metal clamp and a piece of high resistance material is in general very much higher and very much less constant than manufacturers of high resistances have supposed. Of course, conducting wires are fastened to carbon battery plates by means of clamps, but the carbon used in battery plates is a low resistance material. As the specific resistance of the material increases, the contact resistance increases also. Professor Rood, in a recent article,[1] "On the Electrical Resistance of Glass, Quartz, Mica, Ebonite and Gutta-Percha," says: "Mere mechanical contact is not sufficient to abolish contact resistance, which is apt to be quite large." In making this statement Professor Rood was discussing *millions* of *megohms*. The point to the quotation is that contact resistances may be quite large, even in comparison with very large resistances, and that contact resistance increases with the specific resistance of the material used.

About three years ago I had occasion to test a graphite megohm which had been prepared by rubbing a pencil point upon a strip of ground glass. The terminals were strips of tin foil, fastened on with clamps. The manufacturer had tried to eliminate the inconstancy so common in graphite resistances by making the line of graphite very broad, and, of course, proportionally long. I subjected this megohm to a large number of tests, comparing it with platinum film resistances,[2] and also with a wire resistance made by Elliott Bros. The only conclusion at which I could arrive was that the resistance of the graphite megohm varied as much as ten per cent.—sometimes as much as three or four per cent. within a few minutes.

In a paper read before the British Association in September, 1896, Mr. W. M. Mordey[3] offers an improvement upon the ordinary graphite line, in the form of a strip of cartridge paper, coated with graphite and burnished. The advantage of this form is said to be its increased cross-section, though I can see no reason why a

[1] Am. Jour. of Sci., XIV., 161.
[2] PHYS. REV., XI., 40–55 and 84–94.
[3] Phil. Mag., S. 5, XLII., 450.

graphite line on ground glass might not have as large a cross-section as the same material on cartridge paper. An objection to the paper is that it is flexible, and the resistance of the conducting layer of graphite changes when the paper bends. Of course the paper may be mounted in such a way as to minimize the probability of bending, but the untrustworthy character of the clamped contacts is still a serious objection. In June, 1901, I measured the resistance of a film of graphite on cartridge paper, prepared by one of the best manufacturing firms in the world, and bearing their name. On the 25th of the month its resistance was 93,850 ohms. Only one measurement was made on that day. On the 26th, a number of measurements were made, with results varying from 94,000 to 97,000. On the 27th the resistance had fallen to 92,090. All the measurements were made with the same apparatus and under the same conditions of temperature, electromotive force, etc. It is my belief that much of the erratic behavior of carbon resistances which has been attributed to small cross-section could justly be charged to poor contacts.

Being thus thoroughly at war with clamp contacts for high-resistance materials, I tried the plan of electroplating copper terminals to the clay-carbon sticks. Of course this could not be done by passing the plating current through the entire stick, on account of its high resistance, but it is very successfully and easily done by passing the current from the anode, through the solution, to a fine copper wire twisted several times around the end of the carbon stick, as in Fig. 2. In this figure, *A* is the anode; *B*, the clay-carbon stick; *C*, the conducting wire; *D*, the surface of the plating bath, and *E*, the glass dish in which the process is conducted. The copper is at first deposited only upon the copper wire, but in a short time it begins to creep out over the surface of the clay-carbon stick and soon forms a perfect coating over as much of the surface as is immersed in the bath. The contact between the copper and the carbon is so close as to fully satisfy the requirements of the case.

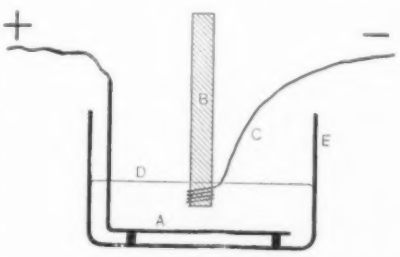


Fig. 2.

I have described these clay-carbon resistances because they have been very serviceable to me and to a number of others who have used them, but I wish also to describe another form of carbon resistance which I find even more satisfactory. It consists of a film of smoke deposited upon a strip of glass. It makes very little difference what kind of smoke or how deposited, though I think perhaps camphor smoke gives results which are a little more nearly uniform than candle smoke or the smoke from a gas jet. The resistance of a film having a given length and breadth, depends, of course, upon its thickness. A film 5 cm. long by 1.5 cm. wide may have any resistance from about 50,000 ohms to about 100 megohms.

As in the work already described, the most serious difficulty to be overcome was the matter of making satisfactory contacts between the carbon and the terminal wires. It was found possible to deposit copper electrolytically upon the ends of the smoke films, but the resistance of the films almost invariably increased abnormally afterwards, many of them rising to infinity. A microscopic examination revealed the fact that the connections between the films and the terminal wires had been weakened or severed by minute cracks in the films, just at the edge of the copper deposit.

Having had an experience similar to this in dealing with platinum films,[1] I resorted to the method which had been so successfully applied to them. It consists in first coating the ends of the glass strip with metallic silver, chemically deposited, and then depositing the resistance material in a uniform layer over the entire surface of glass and silver. The silver tips may be thick enough to stand electrolytic depositions of copper at the extreme ends, or they may even be made so thick that fine copper wires may be soldered directly to the silver.

Tin-foil terminals attached to the glass with shellac and thoroughly baked were tried, but in this case the smoke films which were afterwards deposited were always thinner at the edge of the tin-foil than elsewhere, and in the case of very thin films there was lack of continuity at this point. Even in films of average thickness the resistance at the joint was very high.

[1] PHYS. REV., XI., 53.

Efforts were made to correct this defect in three ways: First, by rubbing down the edge of the tin-foil with very fine emery paper. Second, by overlaying the joint with gold leaf. Third, by brushing smoke into the joint with a camel's-hair brush. In each case the treatment of the joint was previous to the final deposition of smoke. All of these methods gave fairly good results, but none of them equal to coating the ends of the glass with silver by chemical means. The perfection of this method lies in the fact that the portion of the glass which is below the plane of the surface of the silvering bath is heavily coated with silver, while a small portion just above this plane receives silver from only the small amount of solution which rises above the plane of the surface by capillary action. For this reason the silver film, however thick, always terminates in an indefinitely thin edge, so that the layer of smoke which is afterwards deposited, is perfectly continuous and uniform over the entire surface of glass and silver.

A cross-section of one of the tin-foil joints, without preliminary

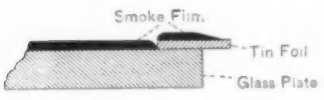


Fig. 3.

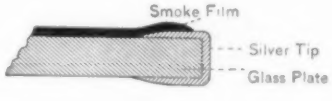


Fig. 4.

treatment, considerably magnified, is represented in Fig. 3. Fig. 4 is a similar representation of a silver-tip joint.

Notwithstanding the perfect smoothness of the silver-tip joint, I was not without apprehension in regard to the possibility of the smoke lying so loosely upon the surface of the silver as to render a high contact resistance possible. I have, therefore, tested these joints in a variety of ways and in almost every case I find their resistance quite negligible.

One of the methods used in testing joints is as follows:

A film is split lengthwise by completely removing the smoke and silver from the central portion of the glass, except at one end, and two terminal wires are provided for the split end. The film is then in the form of a U, with a terminal at each extremity and one at

the bend, as in Fig. 5. The smoke film is represented at *EE*, in the figure, and the silver tips at *DDD*. The resistance is measured

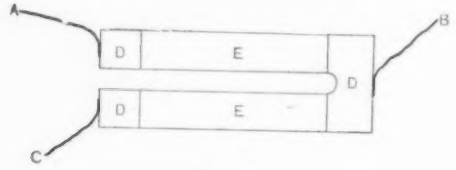


Fig. 5.

between *A* and *B*, and also between *B* and *C*. If the resistance between the smoke and the silver is negligible, the resistance *AC* will be equal to the sum of *AB* and *BC*.

On account of the extreme delicacy of a film of smoke upon glass, it is difficult to handle and use the unprotected films without danger of injuring them. Fortunately, a simple bath in alcohol vapor is sufficient to harden a smoke film to such an extent that it may be further protected by flowing upon its surface a coat of shellac varnish, india-rubber varnish,[1] or melted paraffin; or it is even possible to imbed a film in paraffin without injuring it. This last-mentioned process, however, is not recommended because of the change in volume which the paraffin subsequently undergoes when changes in temperature occur. A thin layer of the material is perfectly safe and quite sufficient to protect the films if ordinary care is used in handling them. If shellac varnish is used as a protecting material, the varnished film should be pretty thoroughly baked at about 110 degrees, in order to remove all the alcohol and water from the varnish. Of course, a film can not be said to be perfectly protected until it is enclosed in a dry wood or hard rubber case and provided with binding posts.

It has not seemed worth while to attempt to adjust these resistances to particular values, on account of the changes in value which would occur afterwards. It really is not very important that they should be adjusted, for it is so easy to make them in large quantities that one may have almost an unlimited variety from which to select. However, it ought to be mentioned here that putting a coat of insulating varnish upon a film increases its resistance considerably.

[1] A solution of india-rubber in carbon bisulphide.

If the film is dipped into the varnish or melted paraffin and then removed immediately and allowed to harden quickly, the protecting layer of insulating material is mainly upon the surface, and the resistance is not very greatly increased; but if the film is allowed to remain immersed in the varnish or melted paraffin for a considerable length of time, the insulating material will soak into the film, separating the particles of carbon from each other and thus greatly increasing the resistance of the film. The preliminary treatment with alcohol vapor *reduces* the resistance, probably because it simply packs the particles of carbon closer together.

Another difference between the tin-foil and silver terminals ought to be pointed out in this connection. When tin-foil terminals are used, even if the joints are successfully made, many of the films which give good results for a time, ultimately increase in resistance, usually to infinity; probably on account of a rupture at the joint, due to the unequal expansion of the different materials. Especially is this true of films which have been coated with shellac or paraffin or even hardened with alcohol. The silver-tipped films are free from this defect. Films which have not been coated with varnish or paraffin, but in which the smoke lies just as it was deposited, are not so likely to suffer in this way, even if they have tin-foil terminals, provided the joints are originally good. I have a number of such films in splendid condition, now over four years old; but of course these unprotected films are always in great danger of being injured by handling.

It has already been stated that the alcohol vapor treatment *reduces* the resistance of a smoke film. This is *always* the case, if the joints are good. An *increase* of resistance under this treatment is always an indication of faulty joints.

The constancy of the silver-tipped smoke films as compared with the clay-carbon sticks with copper-plated terminals is shown by comparing Figs. 1 and 6. The curves in Fig. 1 are for clay-carbon sticks. Those in Fig. 6 are for smoke films. The two sets of curves are drawn on the same scale and are for resistances of about the same range, from one to three or four megohms. It will be seen at a glance that the increase in resistance is very much less rapid in the smoke films.

These curves evidently become practically straight lines after a

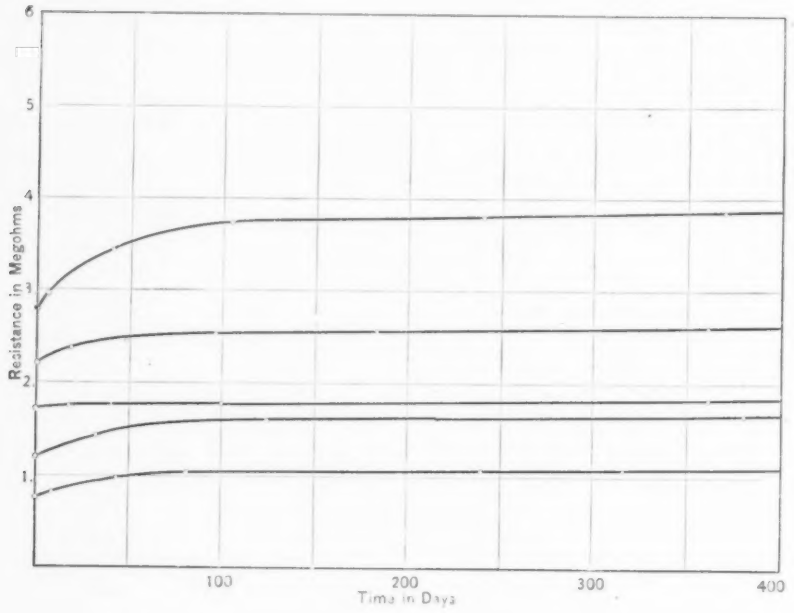


Fig. 6.

sufficient length of time, but they probably never become quite parallel to the X-axis.

In some cases, notably films with tin-foil terminals, it was noted that the resistance would fall when the electromotive force was increased. This fall of resistance amounted in some cases to as much as 4 or 5 per cent. upon increasing the E.M.F. from 1 to 5 volts.

It is believed that the cause of the change in resistance is the closing of the joints at the edges of the tin-foil terminals, by heating, and therefore lengthening the film between the joints. Of course it is true that the amount of heat developed in a film carrying such a small current as these high-resistance films carry, is very small; but, that a small amount of heat will produce a marked effect upon the resistance of one of these joints is made evident by the fact that a poor joint, when used as a bolometer, is capable of detecting temperature changes as small as one ten-thousandth of a degree or less.

The change of resistance with voltage just considered is a change which invariably occurs when the joints are poor.

In comparison with the changes just noted, Table II. exhibits the very small changes which occur under similiar conditions when the silver tips are used instead of tin-foil.

TABLE II.

Film Number.	Resistance in Ohms	E M F in Volts.
53	107030	4.2
	107010	28
	106750	140
54	2848900	4.2
	2848100	28
	2836200	140
55	63391	4.2
	63390	28
	63310	140

These changes, amounting to only a few tenths of one per cent. for 140 volts, are so small as to be fully explained by the temperature coefficient of the carbon; and they leave no room for doubt in regard to the perfection of the contacts.

I have in a number of cases subjected resistances of this kind to a potential difference of 104 volts, alternating, for ten days at a time, without producing any perceptible effect upon them, except to hasten the *aging* process a little.

KNOX COLLEGE PHYSICAL LABORATORY,
GALESBURG, ILL., September 8, 1902.

AN INSTRUMENT FOR DRAWING A SINE CURVE.

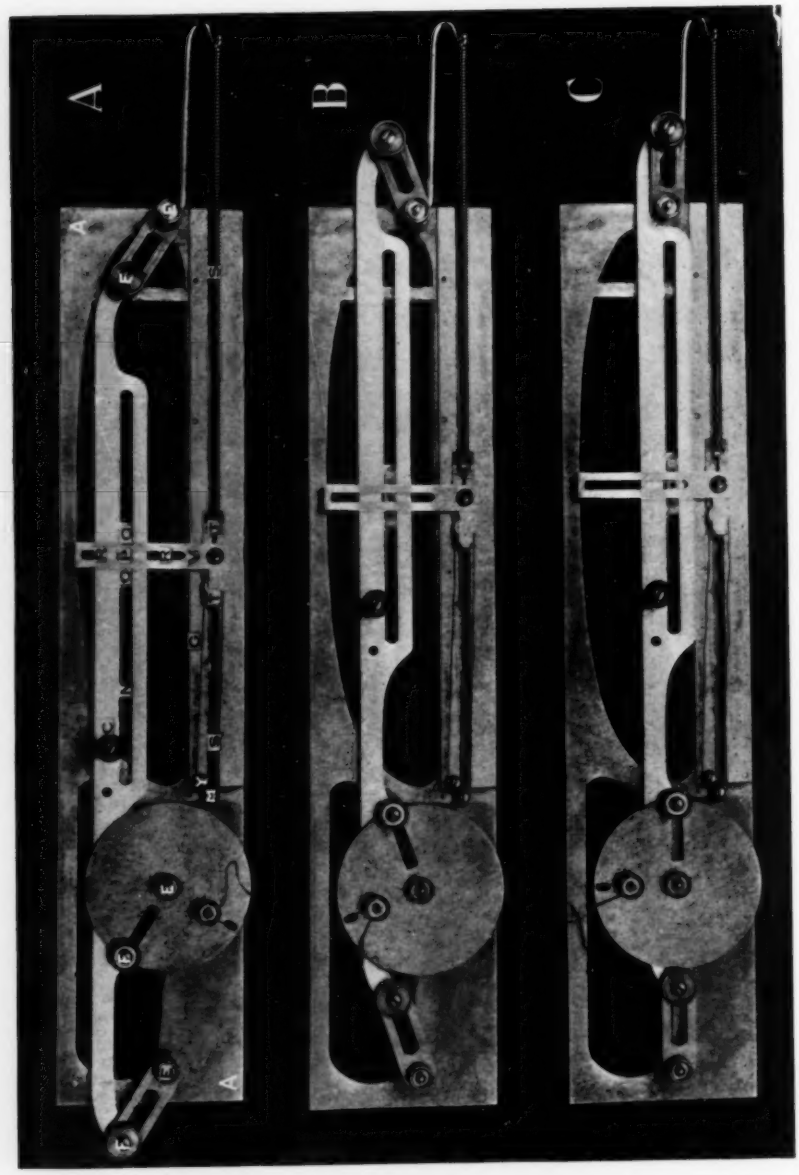
By A. Stanley Mackenzie.

THE instrument here described has been found useful in lecture-room demonstration, and in drawing sine curves of various wave-lengths and amplitudes for any practical purpose, such as addition of these curves to show the effect of phase or the approximate construction of the curve representing a Fourier's series, when one is not fortunate enough to possess an elaborate instrument of the Michelson[1] pattern. Mach[2] devised an apparatus intermediate between the two.

The idea underlying the present instrument may be stated very briefly in the following way. Suppose one of a pair of parallel rules to be fixed. Let one of the arms of the rulers be made of a disc with a groove on its circumference to which one end of a string is fastened and upon which the string winds (or unwinds) as the disc turns. When the disc rotates uniformly the free end of the string moves uniformly and its direction of motion can easily be made parallel to the edges of the rulers; at the same time the movable ruler has the up and down component of its motion simple harmonic. On transferring these two motions to a common point, the latter will move in a sine curve.

How this is carried out will be evident from an inspection of the accompanying illustrations, A, B, C, which show the instrument in three successive stages of its motion. *AA* is a skeleton base-plate of metal. *C* is the movable ruler, cut away where necessary in order that it may pass the pins at *E*, *E*, *E*. *G*, attached to the base-plate, is the fixed ruler. *EF*, *EF*, *EF* are the arms of the rulers and work on the three fixed pins *E*, *E*, *E*, being secured by binding nuts bearing against shoulders. The length of the arms can be regulated by means of the pins and binding nuts *F*, *F*, *F*, moving

[1] Phil. Mag. [5], 45, 85, 1898.
[2] Pogg. Ann., 129, 464, 1866.



MACKENZIE.

in slots. This determines the amplitude of the simple harmonic motion. The middle arm is a disc; one end of the string passes from the groove on the disc to its face and is there secured under a binding nut; the string after leaving the groove passes round a small pulley M to "the slider" V. The movable ruler has in it a slot, N, whose central line coincides with the line EEE when the rulers touch; in this slot moves the guide OPO whose square central portion, P, projects upwards into the slot, RR, in the slider, and carries underneath it the tracing pencil. In the base-plate is a T-slot, SS, in which moves the guide, TT, to which the slider is rigidly connected. At T is attached one end of a spiral spring (the other end of which is fixed to the base-plate) to keep the string taut. The other end of the guide carries a small pulley and a hook at T. The string may be connected directly to this hook, thus giving the longest wave-length, λ, equal to the circumference of the groove on the disc; or it may pass around the pulley T and be attached to a pin at Y, giving a wave-length $\lambda/2$; or it may pass around the pulleys at T and Y and have its end attached to the hook at T (as in the illustrations), giving a wave-length $\lambda/3$. The whole movement is actuated by a handle at C. The base-plate stands on pointed feet, so that when placed on the sheet of paper or cardboard it may not slip. The size of the base-plate used is $2\frac{5}{8}$ by $12\frac{3}{8}$ inches. The string is a bass string of a banjo. The curves which the instrument draws after a little practice are very smooth, and the wave-lengths are remarkably exact submultiples of the circumference of the disc. For smaller wave-lengths the arrangement of pulleys could be carried further, or new discs used.

PHYSICAL LABORATORY,
BRYN MAWR COLLEGE, April, 1902.

THE LIGHT SCATTERED AND TRANSMITTED BY FINE PARTICLES.

BY JAMES BARNES.

WHEN ordinary white light passes through a medium in which are suspended particles whose linear dimensions are small compared with the wave-length of light, the waves of greater length will be more freely transmitted than those of higher refrangibility. This is equivalent to saying that the light reflected, or scattered by such particles of matter consists chiefly of the shorter wave-lengths. The experiments of Tyndall[1] with precipitated clouds formed from various vapors showed that whenever the particles were sufficiently small the light emitted laterally was blue in color and completely polarized, just as the blue light of the sky is polarized. The explanation of the blue of the sky and its polarization was proposed by Lord Rayleigh, and he deduced the law according to which the scattering takes place for waves of different lengths.

The preponderance of the waves of higher refrangibility in the light scattered by small particles can readily be seen by passing a beam of sunlight through a gas flame, where the scattered light is caused by the carbon particles; or when a puff of tobacco smoke or of condensed steam is blown into an illuminating beam.

The following experiments were thus undertaken to ascertain whether any relation existed between the light scattered and transmitted by fine particles suspended in solution, according as the number or size of the particles was increased.

APPARATUS AND METHODS.

The vessel into which the solution containing the particles was placed was 25 cm. long, 18 cm. wide and 14 cm. deep. A beam of sunlight, reflected from a heliostat, was admitted through a circular opening in a screen and passed through the vessel. Two

[1] Phil. Mag., 37, 384, 1869; Phil. Trans., 160, 333, 1870.

spectroscopes were used; one was placed behind the vessel to analyze the transmitted light, and the other perpendicular to the path of the beam to analyze the scattered light. The room where the experiments were carried out was darkened so that no stray light could enter the vessel or the spectroscopes.

The vessel was filled with distilled water to which a few cubic centimeters of a dilute solution of hyposulphite of soda were added. The path of the beam through the solution could be faintly seen, due to the motes scattering a small amount of white light. This scattered light was, however, not of sufficient strength to have any appreciable effect. On the addition of a few drops of dilute sulphuric acid to the solution and after a delay of five to ten minutes, according as the quantity of acid used, a precipitate of sulphur formed. As soon as this precipitation began it proceeded very fast, the solution in a few minutes becoming milky white. This fast precipitation was arrested to a large extent by adding ammonia during the early stages.

When the fine particles of sulphur began to form a blue cloud was seen. The spectrum of this scattered light showed only the violet, blue and green. These colors seemed all to appear at the same time. As the number of particles increased and, therefore, the intensity of the scattered light increased, the Fraunhofer lines became visible. During these first stages the solution outside the path of the beam was a beautiful blue. This is due to the fact that the light incident upon them is not white light, as is the case with the particles in the beam's path, but the light reflected by those latter particles which consists chiefly of the shorter wave-lengths. Thus one might expect that the light scattered by the particles outside the beam's path would be a purer blue, than that scattered by the particles in it.

In this experiment the sulphur particles were not satisfactory, as they formed too fast and aggregated themselves into larger masses, which gradually settled to the bottom of the vessel. After experiments upon other precipitates and solutions it was found that the particles formed when a few drops of an alcoholic solution of mastic were added to a beaker of water were very small, remained suspended in the solution, and did not appear to aggregate themselves into larger masses.

In the vessel was placed about one and a half liters of distilled water and small amounts of mastic solution were dropped in from a burette. The solutions were always well stirred so that the particles were distributed uniformly throughout. In this case as above there was no sharp division in the spectra of the scattered and transmitted lights as the number of particles were increased, and the results were in general the same.

It was noticed that the beam at the beginning of its path was a more decided blue than at the end. When the solution contained a large number of particles the light scattered from the end of the path was a dim red, the blue being all scattered during the first few cubic centimeters. The following method was employed to ascertain what part of the incident light was cut off in transmission and how far each wave-length or portion of a color penetrated solutions containing different amounts of mastic particles.

A beam of sunlight was passed through a Steinheil spectroscope with the telescope removed. The spectrum formed was thrown upon the end of the vessel described above, and after passing through fell upon a scale. The surface of the water in the vessel was adjusted, at the beginning of the experiment, to such a height that half the spectrum passed through the water while the other half passed through the air directly above. The spectrum obtained on the scale was therefore divided by a black horizontal line. By this means one could easily observe the portion of the incident light cut off in transmission through the solution. Looking into the solution from above one could roughly observe how far each portion of a color penetrated a solution.

Results of Observations.

The following tables (Table I., *a*, *b*, *c*, *d*, *e*) were selected from my notes, not because they showed the best agreement among themselves, but because they were the most complete of the many series taken.

The solution of mastic used throughout contained 10 grams of commercial gum mastic dissolved in 100 c.c. of alcohol.

As mentioned above the change in the transmitted light occurred first at the violet end of the spectrum; the blue, green, etc., grad-

ually disappearing as the number of particles increased. It is of interest to note that the red, yellow, and part of the green in the transmitted light kept the same intensity as that in the light which passed above the solution until the violet and the blue had completely disappeared. After this point was reached and on still further increasing the number of particles the red, yellow, and green together gradually lost in intensity. The spectrum then shortened up very slowly.

Observations were taken on clear days only, when the intensity of the sunlight was practically constant during a series of measurements. The scale was always adjusted so that the length of the spectrum was 28 cm. The violet and blue covered the first 14 cm., the green and yellow the next 8 cm., and the red the last 6 cm. One cubic centimeter of mastic solution was added for each observation. In Table I. the position on the scale of the end of the transmitted spectrum is given. As the end of the spectrum was by no

TABLE I.

Amount of the Mastic Solution in Water	Position of End of Transmitted Spectrum on Scale.					Mean.
	a	*b*	*c*	*d*	*e*	
1 c.c.	1.5	1.5	1	1	1.5	1.3
2 "	2	3	2.5	2.5	2	2.4
3 "	3.5	5	3.5	3.5	4	3.9
4 "	6	7	6.5	6	6	6.3
5 "	9	9.5	9.5	9	8.5	9.1
6 "	11.5	11.5	12	11.5	11.5	11.6
7 "	13	13	13.5	12.5	13.5	13.1
8 "	15	14.4	15	15	14.5	14.8
9 "	15.5	15.5		16.5	16	15.9
10 "	16			16.5	16.5	16.3

means sharp it was impossible to make measurements within one half centimeter.

These results are graphically expressed in Fig. 1. The mean readings of the end of the transmitted spectrum are plotted as abscissæ against the amounts of mastic solution in the water as ordinates, and a smooth curve drawn through the points.

From this figure, it is seen, that the slope of the curve is not so great in the blue as in the violet or green. The few observations made with a few more cubic centimeters of mastic added indicated

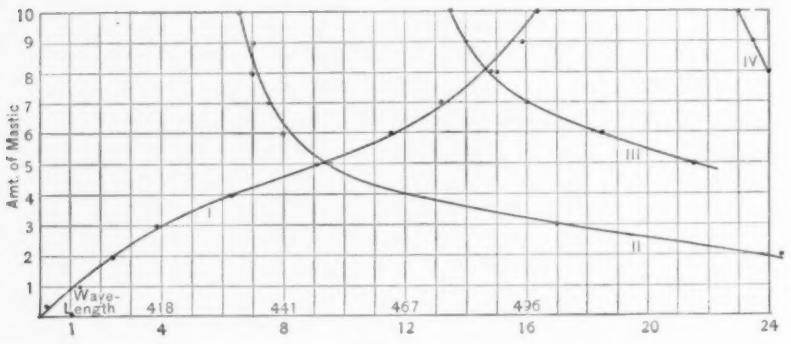


Fig. 1.

that the curve would almost be a straight line parallel to the ordinate axis.

This curve shows that on increasing the number of particles in solution a larger portion of light waves between the lengths 420 $\mu\mu$ and 460 are scattered per equal increase in the number of particles than in the larger or shorter wave-lengths.

The following observations were made to obtain some idea as to how far certain wave-lengths of the incident spectrum penetrated

TABLE II.

Amount of the Mastic Solution in Water.	Distance Penetrated by Given Portion of the Incident Spectrum.		
	$\lambda = 410$.	$\lambda = 431$.	$\lambda = 486$.
2 c.c.	24.5		
3 "	17		
4 "	12		
5 "	9.5	21.5	
6 "	8	18.5	
7 "	7.5	16	
8 "	7	15	24
9 "	7	14	23.5
10 "	6.5	13.5	23

solutions containing different amounts of particles. Table II. contains three series, the first for wave-length about 410 $\mu\mu$ the second for $\lambda = 431$, and the third for $\lambda = 486$. The distances penetrated are expressed in centimeters. The length of the vessel was 25 cm.

The results have also been plotted, curves II., III., IV. The distances penetrated are plotted as abscissæ against the amounts of mastic in solution as ordinates. One will observe what a short distance the violet light penetrates the solution when even the green

goes all the way through. For example, taking the solution containing 8 c.c. of the mastic the rays about the h line ($\lambda = 410$) penetrated the solution 7 cm., those about the G line ($\lambda = 431$) penetrated 15 cm., and those about the F line ($\lambda = 486$) penetrated 24 cm.

We know that if I represents the intensity of the incident light after traversing a distance x of the mastic solution we have

$$dI = -kIdx$$

where k is a constant, generally called the absorption coefficient, depending on the wave-length of the light. On integration,

$$I = I_0 e^{-kx}.$$

For a given wave-length since I and I_0 are constants in above observations,

$$x \propto \frac{1}{k}.$$

The equation of the curve II. gives $xn^{2.5} = \text{constant}$, where n is the amount of the mastic solution present and which may be considered proportional to the number of particles present. Thus, $k \propto n^{2.5}$.

When the incident light consists of a longer wave-length as in curve III., $\lambda = 431$, it is found that the exponent of n approaches unity and thus the absorption k varies directly as the number of particles in solution.

A word in conclusion in reference to the polarization of the scattered light. The light scattered by the particles at right angles to the path of the beam was perfectly polarized. It was noticed that the polarization diminished as the number of particles in the solution increased. In the first few solutions the light from the illuminating spectrum looked at from above was completely cut off by a Nicol's prism with its long diagonal at right angles to the path of the beam. The direction of the polarized light lies therefore in a plane making an angle of 90° with the illuminating beam. On the introduction of more particles after the 5-c.c. solution of mastic had been reached the polarization gradually diminished, a portion of the light passing through the prism in all its positions.

I wish to thank Professor Ames, at whose suggestion these experiments were carried out.

PHYSICAL LABORATORY,
JOHNS HOPKINS UNIVERSITY.

FREQUENCIES OF A HORIZONTALLY SUSPENDED COILED SPRING.

BY F. J. ROGERS.

A LONG coiled spring of large section suspended by cords so that its axis is horizontal is often used to illustrate longitudinal wave-motion. As far as I know this form of apparatus was first described by Weinhold.[1] I have constructed several such "wave-machines" and have found them very effective in teaching the phenomena of longitudinal wave-motion. With the aid of such a machine one can illustrate, advancing waves of compression and of rarefaction; reflection of waves both with and without change of sign; standing waves, including all the modes of vibration of the air in open and closed organ-tubes. With the last machine that I constructed it is easy to obtain standing waves for all the frequencies of which the spring is capable up to and including the tenth mode, counting the fundamental as the first. The frequency of the highest of these modes is less than four, so that these frequencies can be determined numerically merely by counting the vibrations. For a really successful wave-machine of this kind the cross-section of the coil must be great as compared with the diameter of the wire used. In case of the one above mentioned the wire used was No. 18; there were 300 spires, each being 5 cm. in diameter. The length of the spring was 2.4 m.; it was suspended by 25 pairs of threads, each pair consisted of two threads forming an angle of about 35°, each being 60 cm. long.

In using such a wave-machine I have often been reminded of the fact that the wave-motion dealt with is not wholly due to elasticity. It is obvious that the force of gravity must operate to affect the motion, more or less, on account of the suspension being pendulum fashion. Just what this gravity effect may be, is not at all obvious. Upon first thought one might say, that as the gravity

[1] Physikalische Demonstrationen, Zweite auflage, p. 206.

return force is proportional to displacement (when displacements are small) the effect would be the same as if the spring were stiffer, giving as a result an increase of all frequencies in the same ratio. That this is not true is readily proved by determining the frequencies of several modes of vibration. These frequencies obviously do *not* belong to a harmonic series as they would if the vibrations were wholly due to elasticity or as they would if all frequencies were increased in the same ratio by the gravity effect.

The wave-lengths of the different modes of vibration are, however, the same as would be the case if the vibrations were wholly due to elasticity. This is proved by the fact that the *nodes* for all modes of vibration are located just where they would be in the case of purely elastic vibrations. In the case of a coiled spring, whether suspended horizontally or not, the nodes are located with respect to the number of spires, and not with reference to length

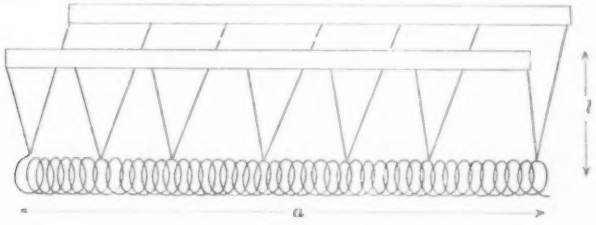


Fig. 1.

simply; for example, a coiled spring free at both ends and vibrating in its fundamental mode will have a node at the middle spire or middle point of the wire. If the spires are not equidistant this may be nearer one end of the spring than the other.

Assuming nodes to be located as in the case of elastic vibrations, it is easy, by the energy method, to compute any or all the frequencies of a horizontally suspended spring. We shall also assume that the motion of any part of the spring is truly simply harmonic. A more general assumption would merely make the mathematical work longer without any advantage, as we are not at present interested in the relative amplitude of the components for any particular method of starting.

Let length of coil be a, length of suspending cords be l, mass of unit length of spring be m, and force required to double the length of the spring be F. See Fig. 1.

Consider the motion of a portion of the spring from a node to the next anti-node and let this length be a/s. If the spring is "free" at both ends or "clamped" at both ends, s may have any of the values, 2, 4, 6, 8, . . . If the spring is free at one end and clamped at the other end s may have any of the values, 1, 3, 5, 7, . . . Let the origin be at a node and x be the distance of a spire of the spring from the origin while y is the displacement of this spire from its normal or equilibrium position. Finally, let y_a be the displacement of the spire at the anti-node. These quantities are represented in Fig. 2. Displacements are of course longitudinal but they are represented in the upper part of the figure as though they were transverse.

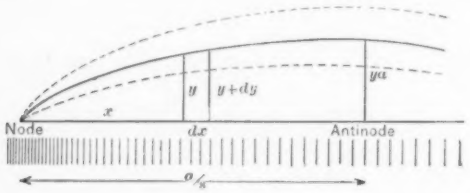


Fig. 2.

According to the assumption already made the velocity of any point of the spring is a simple harmonic function of x, the distance of that point from the origin;

$$\therefore \dot{y} = \dot{y}_a \sin \frac{s\pi x}{2a}. \tag{1}$$

The kinetic energy of the spring is given by the following integral:

$$T = \frac{m}{2}\int \dot{y}^2 dx \tag{2}$$

taken between the proper limits. In the present case this becomes for the whole spring

$$T = \frac{sm\dot{y}_a^2}{2}\int_0^{a/s} \sin^2 \frac{s\pi x}{2a}\, dx = \frac{ma\dot{y}_a^2}{4}. \tag{3}$$

The potential energy of the spring depends upon its elastic distortion and also upon the distance its separate spires are lifted, against gravity, above their normal position.

Consider any portion of the spring of length dx. When dis-

torted its length is $dx + dy$ and the force required to maintain this distortion is $F\frac{dy}{dx}$. The work required to produce this distortion is half the final force times the displacement or

$$\tfrac{1}{2}F\frac{dy}{dx}dy = \tfrac{1}{2}F\frac{dy}{dx}\frac{dy}{dx}dx. \tag{4}$$

The elastic potential energy is therefore

$$U' = \tfrac{1}{2}F\int\left(\frac{dy}{dx}\right)^2 dx. \tag{5}$$

According to the original assumption the displacement of any spire is a simple harmonic function of its distance from the origin or

$$y = y_a \sin\frac{s\pi x}{2a}. \tag{6}$$

Substituting this value of y in (5) and integrating we obtain, for the whole spring,

$$U' = s\cdot\frac{F}{2}\cdot\frac{s^2\pi^2}{4a^2}\,y_a^2\int_0^{a/s}\cos^2\frac{s\pi x}{2a}\,dx = \frac{s^2\pi^2F}{16a}\,y_a^2. \tag{7}$$

The gravitational potential energy of any element of the spring of length dx and mass mdx is the same as the potential energy of a simple pendulum whose bob has the same mass and is displaced horizontally a distance represented by y. This potential energy is therefore

$$U'' = \frac{mg}{2l}\int y^2dx,$$

substituting for y from (6), integrating between the proper limits, we have for the whole spring

$$U'' = s\,\frac{mgy_a^2}{2l}\int_0^{a/s}\sin^2\frac{s\pi x}{2a}\,dx = \frac{mgay_a^2}{4l}. \tag{8}$$

Neglecting the energy dissipated by friction and imperfect elasticity, we have

$$T + U' + U'' = \text{const.}$$

$$\therefore\quad \frac{may_a^2}{4} + \frac{s^2\pi^2F}{16a}y_a^2 + \frac{mga}{4l}y_a^2 = \text{const.} \tag{9}$$

By differentiating the energy equation and simplifying we obtain

$$\ddot{y}_a + \left(\frac{g}{l} + \frac{s^2\pi^2 F}{4ma^2}\right) y_a = 0. \tag{10}$$

From (10) we have for the frequency equation

$$N_s = \frac{1}{2\pi}\sqrt{\frac{g}{l} + \frac{s^2\pi^2 F}{4ma^2}}. \tag{11}$$

If s be given the values 1, 3, 5, 7, ··· or the values 2, 4, 6, 8, ···, the values of N_s do not vary in the same or any multiple ratio. Hence the frequencies of a horizontally suspended coiled spring do not form a harmonic series.

If in equation (11) g/l be put equal to zero we shall have the frequency equation for a coiled spring uninfluenced by gravity; if the other term under the radical be put equal to zero we have the frequency of the gravity pendulum; calling the latter N_0 and the former N_s', (11) may be written in the following form:

$$N_s = \sqrt{N_0^2 + N_s'^2}.$$

The frequency equation (11) may be verified by determining frequencies experimentally and by computing them from the constants of the spring: Instead of doing this I have adopted the following method: If N_0 represents the pendulum frequency of the suspended spring (11) may be written as follows:

$$N_s = \sqrt{N_0^2 + s^2 k},$$

in which k is a constant. Giving s some particular value, say 2, we have

$$k = \frac{N_2^2 - N_0^2}{4}$$

$$\therefore N_s = \sqrt{N_0^2 + \frac{s^2}{4}(N_2^2 - N_0^2)}. \tag{12}$$

If the two frequencies N_0 and N_2 be determined experimentally all the other frequencies can be computed by substitution in equation (12). This was done for all frequencies up to N_{10}, and the results compared with experimental values.

Frequencies of Horizontal Coiled Spring.

N_s	Observed.	Computed.	N_s' Computed.
N_0	.697	—	.000
N_1	.785	.790	.372
N_2	1.020	—	.744
N_3	1.31	1.31	1.116
N_4	1.63	1.63	1.49
N_5	1.99	1.98	1.85
N_6	2.35	2.33	2.23
N_7	2.69	2.67	2.60
N_8	3.08	3.05	2.98
N_9	3.42	3.42	3.35
N_{10}	3.77	3.78	3.72

In the last column of the above table are given, for the sake of comparison, the frequencies which the spring would have had if the vibration were wholly due to elasticity. As was to be expected the actual frequencies of the higher components are not much influenced by the pendulum suspension. This is not the case, however, with the lower components.

The differences between the computed and observed frequencies are well within the errors of observation. The latter were probably in the neighborhood of a quarter per cent. Considering the crudeness of the spring and the assumptions made in the mathematical demonstration it was not thought worth while to strive for greater accuracy in the experimental verification. Besides the assumptions explicitly mentioned, the use of the calculus implicitly assumes that the spring is continuous like a rod or tube instead of being made up of widely separated spires, also that the method of suspension is continuous instead of being limited to a few widely separated threads.

STANFORD UNIVERSITY,
May, 1902.

OPTICAL NOTES (1): ON A SPECTROSCOPIC APPLICATION OF TALBOT'S INTERFERENCE FRINGES

BY K. TSURUTA.

A HAND spectroscope with direct vision is often added to some optical measuring instruments without scale, and to make measurements with it by using an artificial luminous source such as petroleum lamp or coal-gas flame, it is only requisite to mark out with a sufficient degree of accuracy points of definite wave-lengths in the continuous spectrum produced. This can be accomplished by the elegant method of Kirchhoff and Bunsen, or in a variety of other ways. One of these, depending on Talbot's interference fringes, will be here described. It requires quite simple apparatus, but is sufficiently good for certain kinds of investigation, especially for students' practice in a physical laboratory. I confine myself to my experiments made in connection with Pulfrich's "Totalreflectometer for chemists," but it is believed that the method can be applied with advantage under other circumstances.[1]

My spectroscope was of exactly the same type as that described by Pulfrich in his paper on "Das Totalreflectometer und seine Verwendbarkeit für weisses Licht," but without the scale. To produce the fringes, quite ordinary cover glass plate was employed, being put between the prisms and Ramsden's eye-piece, after having been fixed with wax, as shown in the annexed figure, to a small tube fitted to the eye-piece. Its straight edge (obtained by simply cutting with diamond) could be by rotation made parallel to the slit of the spectroscope.

In counting the fringes, I took as origin the bright Na lines appearing in the continuous spectrum of a luminous flame of coal gas, in which a piece of fused sodium chloride was suspended.

The wave-lengths corresponding to the fringes were indirectly determined by measuring first the angles of emergence from the

[1] For example, Rubens, Wied. Ann., Bd. 45, p. 238.

total reflectometer prism for the principal Fraunhofer's lines, and then those for the fringes. The former were platted with reference to the known wave-lengths of the lines and the values for the fringes were obtained by interpolation. Since an angular difference of only 1′ could be measured on the divided circle, the angles of emergence had to be carefully determined; also smoothing of the curve and interpolation had to be repeatedly and carefully done.

The following is an example:

λ corresponding to the fringes.

No. of Fringes.	Using a Prism of Crown Glass.	Using a Plate of Flint Glass.	Values Adopted.	No. of Fringes.	Using a Prism of Crown Glass.	Using a Plate of Flint Glass.	Values Adopted.
−18	0.000670	0.000672	0.000670	+ 3	0.000580	0.000580	0.000580
−15	656	656	656	+ 6	569	568	569
−12	643	642	642	+ 9	559	557	558
− 9	629	629	629	+12	548	547	548
− 6	616	616	616	+15	539	538	538
− 3	604	604	604	+18	528	529	529
0	591	592	592	+21	520	520	520

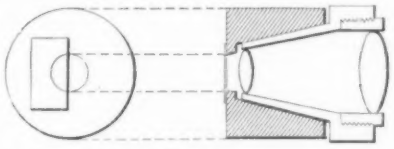


Fig. 1.

The indices of refraction of the total reflectometer prism for the wave-lengths corresponding to the fringes may be interpolated from the values given by Pulfrich for the principal Fraunhofer's lines, or what comes to the same thing, determined by making use of a body with known indices of refraction for different wave-lengths. I took a quartz cube, for which Mascart's measurements were assumed, and obtained the following values, with which Pulfrich's perfectly agree.

Indices of refraction of the total reflectometer prism corresponding to the principal Fraunhofer's lines (18°.8 C.).

	A	*B*	*C*	*D*	*E*	*F*
Tsuruta.	1.60702	1.60854	1.61032	1.61511	1.62136	1.62705
Pulfrich.		1.60854	1.61031	1.61511	1.62141	1.62703

Whence by graphical interpolation I obtained the following values :

Indices of refraction of the total reflectometer prism corresponding to the fringes (18°.8 C.).

No. of Fringes.	λ	Index of Refraction.	No. of Fringes.	λ	Index of Refraction.
−18	0.000670	1.6095	+ 3	0.000580	1.6161
−15	656	1.6103	+ 6	569	1.6169
−12	642	1.6112	+ 9	558	1.6179
− 9	629	1.6121	+12	548	1.6190
− 6	616	1.6130	+15	538	1.6201
− 3	604	1.6139	+18	529	1.6211
0	592	1.6149	+21	520	1.6223

Now to test the method, indices of refraction of different substances were determined; the following example gives the means of two series of measurements made with a plate of crown glass on different days, but at nearly equal temperatures (18°.8 and 19°.4C.), once with the sun's light and then with a coal-gas flame.

Solar light.

A	*B*	*C*	*D*	*E*	*F*
1.52619	1.52706	1.52808	1.53087	1.53406	1.53708

Coal-gas flame.

No. of Fringes.	Index Obs.	Index Interp.	No. of Fringes.	Index Obs.	Index Interp.
−18	1.5275	1.5276	+ 3	1.5313	1.5312
−15	1.5280	1.5281	+ 6	1.5316	1.5318
−12	1.5285	1.5286	+ 9	1.5323	1.5323
− 9	1.5290	1.5291	+12	1.5328	1.5329
− 6	1.5295	1.5296	+15	1.5334	1.5334
− 3	1.5300	1.5301	+18	1.5339	1.5340
0	1.5306	1.5307	+21	1.5346	1.5346

The numbers in the column "Index Obs." were concluded from the measured angles of emergence, while those in the column "Index Interp." were interpolated from the curve obtained by plotting the values of the first table on the assumption of the formerly determined wave-lengths for the fringes.

GÖTTINGEN, August 4, 1902.

NEW BOOKS.

A Treatise on Geometrical Optics. By R. A. Herman. 8vo. Pp. 344. Cambridge, University Press, 1900.

This treatise differs in several essential features from its predecessors. Like most recent English works on geometrical optics, it is written from the standpoint of the mathematician, and is for that reason somewhat lacking in the practical applications of this branch of mathematical science; yet it contains much that is new, and is perhaps the most elaborate work on the subject that has appeared.

A new departure in method is the determination of the properties of an optical system symmetric about an axis by means of the *angles* made by the rays with the axis, instead of the *position* of the luminous point and its successive conjugate foci. As the author informs us, this method is implicitly contained in Cotes' theorem (1738) of the apparent distance, *i. e.*, the distance from the eye at which the object must be placed when viewed directly, in order to subtend the angle which it appears to subtend when seen through the system. An expression similar to Cotes' for the apparent distance is derived for the angular magnification, and this in turn is brought into relation with the linear magnification by aid of the formula of Lagrange-Helmholtz. The Gaussian principal points and the nodal points are then at once given by placing the linear and angular magnifications respectively equal to unity, the principal foci and focal lengths (and powers) by equating the original and final angles of divergence respectively to zero. The cardinal properties of the system are thus determined by an expression involving only simple geometrical distances, while the analytical methods of Gauss require a lengthy calculation of continued fractions.

The extension of this method to angles of divergence the squares of whose circular measure cannot, as in the above, be neglected, leads to formulæ of great elegance for the spherical aberration of axial points, and further to such for the aberrations of extra-axial points, *i. e.*, for a small object perpendicular to the axis.

The part of the work which the student of optics will especially welcome and which will probably appeal most strongly to the physicist, is the application of the Principle of Least Time to the solution of optical problems. This principle dates in its inception from Fermat's well-known theorem, that the reduced path (time) of a ray between two points separated by any number of media of refractive indices μ is a

minimum ; that is to say, the ray takes a course such that the variation of the line integral $\int \mu ds$ vanishes, s being the actual length of path. Its usefulness was extended by Hamilton through the introduction of his "Characteristic Function," a form of the reduced path containing only the coördinates of the points and the constants of the orthotomic surfaces (wave-surfaces) of the rays, but which leads, unfortunately, to great mathematical difficulties. Maxwell, using a still different form for the reduced path, was able, by an ingenious method, to discuss the properties of a symmetrical optical system for small pencils and also for pencils of finite divergence, thus finding its cardinal properties as disclosed by the theory of Gauss, and giving, besides this, formulæ for the various aberrations to a second approximation.

The results of Hamilton and particularly those of Maxwell were nearly forgotten ; in fact, on the Continent they appear to have been wholly unknown and it is very commendable of the author to have again called attention to them and, by his masterly treatment, to have made them more readily accessible.

The subjects of the other chapters correspond to those of previous works and need not be enumerated here.

A deficiency of the book is the brevity with which the problems of practical optics have been dealt with. An exhaustive treatment of the theory of optical instruments is doubtless beyond the province of a text-book on geometrical optics ; nevertheless a more thorough exposition of some of the more recent triumphs of constructive optics, *e. g.*, of photographic objectives which are entirely unnoticed, would have added to the value of the book and awakened the reader to some of the actualities of modern practice and would have also indicated to the mathematican the needs of the working optician. For, in this as in so many fields of applied science, it is only through theory and practice working hand in hand that the highest results can be attained, a lesson that the English-speaking peoples appear to be slow in learning.

Apart from this, the volume has much to commend itself. Although the demands made upon the mathematical knowledge of the reader are not inconsiderable, yet the treatment is clear and uniform throughout and, particularly if already somewhat conversant with the subject, he will find its perusal very suggestive.

H. C. LOMB.

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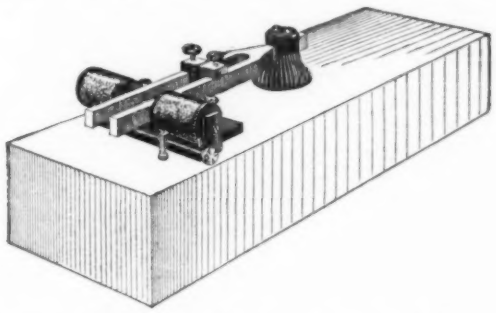

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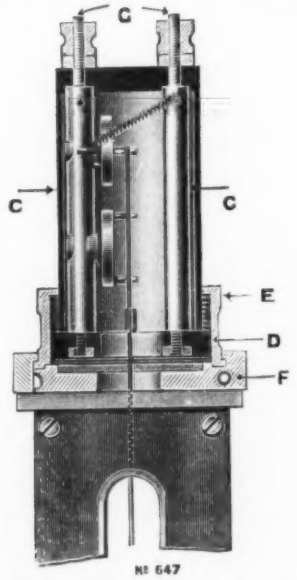

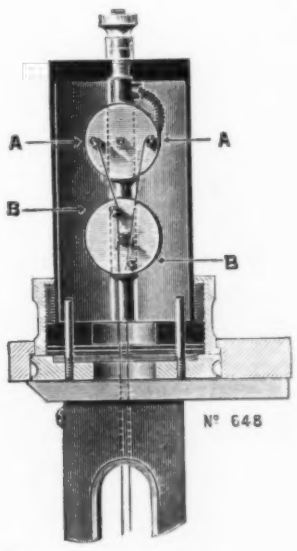

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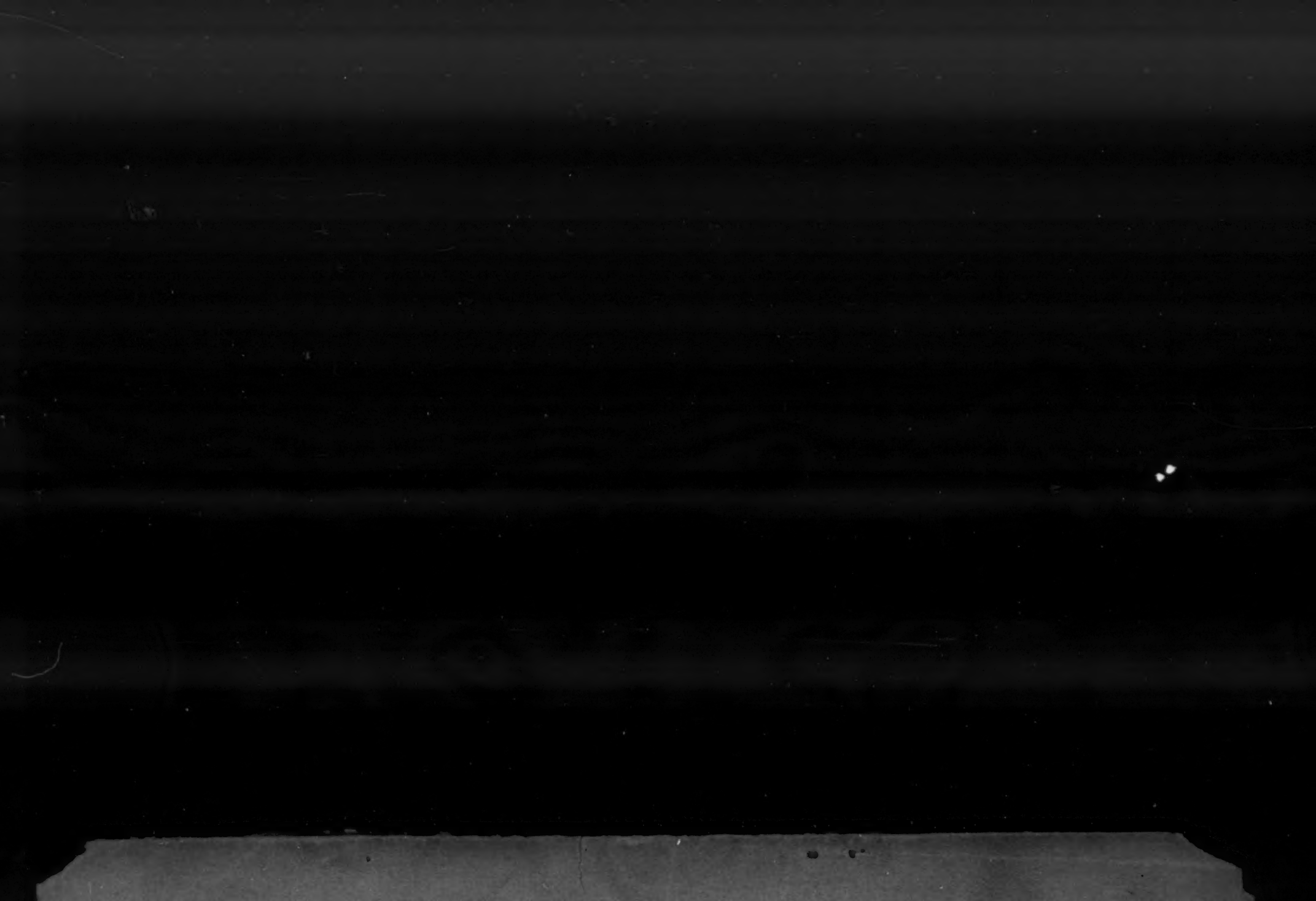
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